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WHAT IS THE VALUE OF WOODWORTH'S PSYCHOLOGY?

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It is difficult to set out Mr. Woodworth's teaching on "stimulus-response psychology". For he does not elaborate a system: he does not venture an opinion as to the nature and structure of "mind", leaving such questions to the metaphysician: he would, I presume, resent being labelled as one who holds a mechanistic interpretation of conduct, though there does seem a distinct flavour of mechanism in his teaching: he does not take sides on the dispute between vitalism and materialism, though there are passages in his writings which seem to imply a denial of a radical and unbridgable difference between the action of living and non-living things. He is of opinion that we can make valuable discoveries in psychology without broaching any such philosophical questions as that of causation, or the distinction between a cause, an occasion and a condition; neither, he would say, need we ask what is the mind and what is the relation between mind and the body which conditions its activity. Further, being an evolutionist—though he does not explicitly defend this theory—he is of opinion that there is no radical difference between any of the mental activities of a human being and of an animal; consequently he attaches much value to the study of animal psychology as giving us an insight into the working of the more developed human mind.

For him psychology, though a "pure science", is yet essentially practical in its aim. For, this study is motived by the desire to help the educationalist, the criminologist, the alienist, the sociologist, the employer of labour, the teacher of morals and religion. "For the psychologist conduct is a matter of cause and effect, of natural law. His business is to know the laws of that part of our nature which we call human nature, and to use these laws, as far as discovered, for solving the problems presented by the human individual or the group" (p. 18).

Now Mr. Woodworth holds that human conduct can be fully described in terms of the individual's "response" (or "reaction") to "stimuli". By this he does not mean merely to deny the complete spontaneity of any mental act: he is not combating the position that our wishes and perceptions and aversions and resolutions may spring into existence for no reason at all: he does not merely mean that every mental act must have something to account for its occurrence, and that this something is a "stimulus". He claims more than this, viz., that he can lay his finger on those precise stimuli to which we respond. These he carefully lists out and describes, and then sets out to show how every act of the individual is a "response" to one or more of these stimuli. He thus professes to discover *how* we learn and remember and perceive and will and resolve *et cetera*; for all these actions are "responses to stimuli" and follow certain laws. Such a discovery would of course be immensely useful, if it were true.[1]

My main contention will be that if Mr. Woodworth does not go beyond his own list of stimuli, then it can be shown that there are many mental acts which are not, and could not be, the corresponding "responses". I shall further urge that the connection between the alleged stimulus and the response is, in many cases, so very remote and implies so many intervening operations, that the explanation of the act in question as being a "response to a stimulus", is not an explanation that has any value, practical or theoretical. I shall also submit reasons for holding that Mr. Woodworth incorrectly describes many facts of mental life when making them fit into his theory: that he omits most important features which run counter to his view: that his examination of animal behaviour is vitiated by a faulty principle of observation: that in some very fundamental investigations he quite unwittingly blunders in his logic.

I am emboldened to attempt this very drastic criticism; for the author welcomes it:

> "Science does not like to have unverified hypotheses lying around loose, where they may trip up the unwary. It is incumbent on anyone who puts forward a hypothesis to apply it to as many special cases as possible, in order to see whether it works or not; and if the propounder of the hypothesis is so much in love with it that he fails to give it a thorough test, his scientific colleagues are sure to come to the rescue; for they, on the whole, would be rather pleased to see the other fellow's hypothesis come to grief. In this way the rivalry

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[1] Mr. Woodworth's "Psychology, A Study of Mental Life" has gone through many editions. I am acquainted with only the first seven, and it is on these alone that I have based my criticism. I have since learned that there is a tenth edition, which up to this time I have been unable to procure. I would therefore ask my critics to group their findings under two distinct heads: (i) Do the first seven editions warrant my strictures, or have I entirely misread the author? (ii) Do the three later editions make any radical changes in the author's position? In particular, has he expanded his list of *three* stimuli, on whose exclusiveness I have throughout so much relied?

motive plays a useful part in the progress and stabilising of science" (p. 474).

I propose to adopt the rôle, though not the malicious motive, suggested in the case of a "scientific colleague", and "to come to the rescue". For I am of opinion that Mr. Woodworth has not given his hypothesis a thorough test; and further I feel that unless a psychologist squarely faces up to metaphysics, and, from the nature of our mental activities forms some definite theory as to the nature of *that which thinks*, he cannot arrive at any very valuable findings. And still further, any theorising which the psychologist must engage in, implies very definite views on the meaning of substance, a substantial union, causation, relation, activity, *et cetera.* It is the complete absence of all discussion on these vastly important matters that has caused me to hold Mr. Woodworth's theories in suspicion.

I.

STIMULUS-RESPONSE.

"Having defined a reaction as an *act of the individual aroused by a stimulus*, there is no reason why we should not include a great variety of mental processes under the general head of reactions. *Any* mental process is an *activity of the organism*, and it is aroused by some *stimulus*, external or internal; therefore it is a reaction" (p. 45).

(1) By the phrases "activity of the organism", and "act of the individual" the author means that any mental activity is to be attributed not to one part or element of the organism, but to the individual as a whole: that, *e.g.*, a sensation, a wish, a concept, a resolution, a dislike, a perception of a relation between two things—these belong to the whole man, rather than to any part of him. We say they are *his*, rather than that they belong to his brain, or his mind, or to a particular "faculty".

Now there is an obvious element of truth in this which no one would deny.

(2) The assertion: "any mental process is an activity of the organism, and it is therefore aroused by some stimulus external or internal; therefore it is a reaction", is dangerous because vague. It might easily be taken to imply that the reaction always bears the same relation to the stimulus—which would be very difficult to maintain. Take for instance two cases: The admiration of patriotism when I read of a soldier's brave deed, and the sensation of pain when a pin pricks my finger. In the former case there is a mental activity which is a blend of sentiment and of reason; this is occasioned by the reading of certain facts. In the latter case there is a painful state of consciousness; this is directly caused by the

stimulation of the nerve. Now, to say that both these mental states are just "reactions" to stimuli implies that the reading of an incident and the being pricked by a pin are each related in the same way to the consequent reaction or "response". Whereas, the fact is that in the one case a series of rational acts containing judgments of value, intervene between the stimulus and the reaction which is an admiration of patriotism; in the other case nothing intervenes between the nervous stimulation and the resultant sensation.

To "explain" mental activity by calling it a "response to a stimulus" gives us very little information about the activity in question. It would be like "explaining" the back-firing of a motor car and the speedometer's continuing to point to 60, by saying that both were "reactions" of the car to the stimulus of petrol. We should like a little more information concerning the proximate causes of each of these reactions, if we are going to control them.

Now it seems to me that Mr. Woodworth fails to give us just that information about mental life that a mechanic would fail to give us about a motor car who should group all its widely different movements as "responses to the petrol". If we confined ourselves to these generalities, we should not be able to effect repairs when we met with a breakdown. Mr. Woodworth lists out various mental activities (sensations; associations; the retention, recall and recognition involved in memory; willing; reasoning; appreciating) and these he classifies and co-ordinates, and connects with stimuli. He never raises the question: Is the feeling of a sensation of the same nature as the drawing of an inference? Is the proximate cause the same in each case? He is satisfied to group them together as "responses" of the organism. In this he is like the motor mechanic, intent on studying motor activity, yet never asking himself whether the "response" of the speedometer and of the hind wheel were produced in the same way. I suspect that a dread of "faculty psychology" is the explanation of this strange omission on Mr. Woodworth's part.

We must take this occasion of noting very carefully what the author means by a "stimulus"; it is the very core of his teaching; and it will be necessary later on, when studying his various analyses of mental acts and states, to have clearly in mind his own account of the three kinds of stimulus. They are:

(*a*) An *"external stimulus"*. "Typically, the stimulus is an external force or motion, such as light or sound, striking on a sense organ."

(*b*) *"Internal stimulus."* "There are also the internal stimuli, consisting of changes occurring within the body and acting on the sensory nerves that are distributed to the muscles, bones, lungs, stomach and most of the organs" (p. 47); *e.g.* hunger, thirst, fatigue.

(*c*) *"Central stimuli."* "It seems necessary to recognise another sort of stimuli, which act *directly on the nerve centres of the brain.* . . . We have simply to think of one nerve centre arousing another by means of the tract of axons connecting the two. Say the auditory centre is aroused by hearing someone mention your friend's name, and this promptly calls up a mental picture of your friend; here the auditory centre has aroused the visual."

The following sentence which seems to set up a general law or principle, is most important:

> "What happens in a train of *thought* is that first one group of neurons is aroused to activity, and then this activity, spreading along the axons that extend from this group of neurons to another, arouses the second group to activity; and so on. . . . All the evidence we have would indicate that the brain is not *'self active'*, but *only* responsive; but once thrown into activity at one point, it may successively become active at many other points, so that a long series of *mental operations* may follow upon a single sensory stimulus" (p. 49).

Thus when he speaks of a "thought being a stimulus that rouses an emotional response" (p. 48), we must remember that he is speaking of this "central stimulus". Then when "the thought of my enemy makes me angry", the process of the mental activity is as follows: The thought of the enemy has been aroused by the stimulation of the nerve centre connected with that thought; this nerve centre when excited arouses, by means of a tract of axons connecting the two, that other centre which is connected with the emotional response of anger.

(A) Many would willingly subscribe to one of the main contentions in the passages cited under (*c*). For there is no difficulty in admitting as theoretically satisfactory this physiological explanation of the association of ideas. W. James has elaborated it with his usual forcefulness: and it offers a satisfactory substitute for Freud's arbitrary explanation of dreams.

(B) But, I submit, grave exception can be taken to the notion of "central stimuli" as explaining *all* trains of thought. Let us test the theory and see whether it squares with our experience. The author's contention is that the mind is not self active,[1] but instead is always a "response".

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[1] True, his words are: "The *brain* is not self active." But the whole point of the theory is that all mental activity is a "response"; therefore there is no mental self activity. Hence the words in the text do not misrepresent the author.

The working of a "central stimulus" could be set out as follows: An "external stimulus" is experience to which the "response" is the consciousness of an external object. This consciousness entails an activation of one nerve centre, and this latter through its axon stimulates another centre whose corresponding thought then comes as a "response". The nerve centre last set acting, now stimulates through its axon still another centre, whose corresponding thought also comes as a "response". Thus "a long series of mental operations may follow upon a single sensory stimulus".

I suggest the following points of adverse criticism:

(i) This analysis of the process of thinking likens our connected thoughts to the crackers that explode one after another when a light is thrown into a bunch of them. It inevitably implies—or at least very strongly suggests—that our chain of thought is random, and is not carefully guided. Why? Because there is no stimulus for the directive operation, which, therefore, cannot be a "response". One naturally asks what is the stimulus that accounts for the response in a case like this: After hearing the witnesses for the defence and the prosecution, a Judge defers sentence. He is not certain which of the witnesses are reliable. Carefully reviewing the facts at night, he concludes in favour of the defendants. In the morning however, *without any additional evidence and without any new considerations*, he changes his mind and decides that it is more prudent to reject the reliability of a certain witness—thinking over the man's manner of speech, and the lack of probability in his story, the Judge finally discredits his statements.

Now here it must be admitted: (*a*) that precisely the same stimuli were present in the evening and in the morning; and yet an entirely different response ensued on each of these occasions. I ask: "How square these facts with the stimulus-response theory?" (*b*) Over and above the activity of a stimulus in provoking a response, there was an activity which *directed* thought: and an activity which *selected* a particular response for further examination: and an activity which *evaluated* a particular response. I ask: "Where is the stimulus which provoked this complicated response?" Beyond all doubt we seem forced to admit that mere stimulation (external, internal, or "central") leaves unexplained and unexplainable the response of the Judge when he passes sentence in the court in the morning.

(ii) Mr. Woodworth completely loses sight of our perception of *meaning.* For when I understand *why* the square on the hypotenuse of a right angled triangle is equal to the sum of the squares on the other two sides, introspection tells me that I am doing something more than *responding* to stimuli—whether external, internal or central. It is very poor logic to suggest that because each of the steps in my reasoning has a corresponding movement in the brain, these movements cause me to arrive at the conclusion. All the evidence of consciousness points to my being "self-active"—for I *endeavour* to see the bearing of one truth upon another: I *try* to see relations between them.

(iii) The stimulus-response theory does not offer a very convincing explanation of a long sustained mental effort: "We have to *suppose* that a centre, once aroused to activity, and prevented from complete discharge, remains active by virtue of energy dammed up in itself" (p. 83). Test this suggested explanation by applying it to the activity required in writing a book: A year's abstention from distracting or unnecessary amusement: a year's constant self discipline: a year's watchfulness over odd hours that would easily be frittered away: a year's effort to withstand the appeal of many legitimate interests in reading and study—the constancy of this immense flow of vital power which takes such toll of the whole organism is all due to and comes from a *nerve centre in the brain cortex,* with its supply of "dammed up energy"! I ask: Is this meant as serious theory? Is this steadfast determination of an author at his work nothing more, in its structure and its essence, than the dammed up energy of a nerve centre? It would be a precious discovery for a would-be author if he could hit upon a "stimulus" (external, internal or central) that would excite this most amazing nerve centre and ensure that the book would just *get* finished in time. We are forced to admit the plain fact that stimulus-response psychology completely fails to explain the facts. In spite of Mr. Woodworth, the mind *is* "self active" and not merely "responsive"—but Mr. Woodworth seemingly cannot admit of a mind; this would be too much like the "old faculty psychology".

(iv) And relations? We most certainly do perceive relations between things and between thoughts and between principles. Now I urge most strongly that here my perception cannot, by any stretch of language, be called a "response to a stimulus"—for the simple reason that there *is no stimulus.*

Let us first take relations perceived to exist between things: My two beloved pipes are in front of me. Through the eye I receive two and *only two* stimuli; the difference in size between the two pipes is not itself a *third* object, providing a third stimulus. How then account for my perception of one pipe's being larger than the other? Simply by saying that I hold together in consciousness the two objects, and by an act of comparison see how one is ordered to, or bears upon, or is related to the other. Now, this act of comparison is not a response to a stimulus; for it is not the response of the eye to either or to both pipes, since in neither case is there on the retina anything which is an impression of the *difference* between the pipes. The activity of comparing the objects is not that of the eye,[1] but of an entirely different agent, *viz.,* the mind.

Mr. Woodworth's whole theory of stimulus-response breaks down; for he can never account for this "response" which has no stimulus. If we have nothing but responses to stimuli, then we could never have one consisting of the perception that one pipe is larger than another.[2]

Let us next take perception of relations between truths or principles or thoughts. Here the same line of criticism is even more effective. I perceive, *e.g.,* that since a triangle must have its interior angles equal to two right angles, a particular figure with two and a half right angles cannot be a triangle. It is simply dead against the verdict of consciousness to analyse this process of rational inference in terms of response to stimuli. Why? Because no catenation of nerve modifications, corresponding to various thoughts, can produce an entirely *new* thought. The most it can do is to bring into consciousness an old one, or a combination of old ones. When a third nerve centre is stimulated as the result of two connected previous responses, there cannot be a third thought, unless this is *already* connected with the stimulus. How, then, can my chain of geometrical thought be simply a chain of responses to stimuli?

(v) The theory, I submit, breaks down even in some of the very cases cited as illustrations of it. We are told *e.g.* (p. 48): "A thought may arouse an emotion, as when the thought of an enemy, suddenly occurring to mind, makes me angry."

I admit, of course, that there are such cases of what may be called automatic responses, and where the author's explanation may be near enough to the truth. But he should take account of emotional responses which will not square with his theory. Here is one at random: Let us suppose that I have never seen a man brutally punch his wife in the face; and that today for the first time, I witness the horrid scene. I am at once angry. Now is this state of mind merely a response to the stimulus? Is it not due to my seeing more than my eye can see, *viz.*, the utter lack of order or proportion in such an act? From the very fact that it was my first experience, the mere sight as such can never have been connected with this emotion. We must go elsewhere than to stimulus-response theory for an explanation of my anger. We have to admit that the mind can see meanings and relations: that it can form standards of conduct: that it can compare acts with such standards. But all this is something utterly different from the mere "responding to stimuli".

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[1] In common speech we talk of comparing "with" the eye; what we ought to say is "by means of" the eye.

[2] It would be interesting to follow a student of animal behaviour in his proof—if he attempts to give one—that an animal does perceive differences as such. The animal reacts differently to different objects. But this is not, and could not be offered as, a proof that it is aware that one object is different from another. It is precisely in such cases of animal behaviour that the psychologist is all too prone to read his mind into the animal's.

To express this criticism otherwise, we could say that Mr. Woodworth's theory can offer no explanation of this contrast: I see a man calmly walk up to a pet lamb on the lawn and slit its throat; I feel angry and despise the man for his act. I see a man calmly walk up to a pet lamb in the abattoirs and slit its throat; I feel neither anger nor do I despise the man. Surely the stimulus of the lawn and that of the abattoirs cannot be so very different as to account for the contrast in the emotional response. Mere response to stimuli does not suffice; there is a mental state for which there is *no* stimulus; for the emotion is due not to a stimulus (whether external, internal or central) but to a rational act of mind.

(vi) The theory breaks down even more hopelessly when we apply it to cases where I "respond" by seeing an obligation. For example, "I want to get to town in ten minutes; therefore I *must* do something other than walk there." To call my perception a response to a stimulus is simply to label a distinctively human activity with a vague and meaningless name. It explains nothing and it has no use.

II.

REASONING AND INFERENCE.

Let us examine the author's treatment of reasoning and inference. I am surprised at the brevity of his discussion. For one would have thought that this distinctive and most valuable possession of man[1] would, when studied carefully, enable us to learn something of the nature of the mind itself and of its relation to the brain; it might enable us to learn something of the origin and the destiny of that which thinks.

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[1] Even the evolutionist would admit this to be in a sense true.

But the author's treatment is the briefest—some sixteen pages, of which nearly ten comprise illustrations and anecdotes.

In the second place it is worth while to look into the author's examination of reasoning and inferring; for it enables me to answer an objection that may have occurred to the reader of the previous pages. Have I not misread Mr. Woodworth by supposing that his remarks, in the passages criticised, are intended to apply to *all* trains of thought, instead of only to those which he uses as illustrations? No. For he speaks of a train of reasoning and of inference as being a response to a stimulus, and he has not given us any other kind of stimulus than the three already listed out. I conclude then that he does intend the stimulus-response theory to apply to *all* trains of thought, including that of reasoning. Let us now examine his theory and its application.

Reasoning:

(1) He openly admits that reasoning is different from sense-perception (p. 462); and with this I am in total agreement.

(2) But reasoning, he tells us, is a form of *"perception"*, and the difference between them is not so radical as it might seem. The activity is allegedly of the same sort in each case; it is only the position of the "central stimulus" that is different; the relation to the "cortical stimulus" in the brain, is precisely the same in sensation and reasoning. The author's theory is this: In sensation proper (sight of an object) we have a nerve-stimulation which itself terminates in a conscious mental state, *i.e.,* we see when the optic nerve is stimulated by light. "Conscious sensation is the response of the part of the cortex that *first* receives the nerve current from the sense organ stimulated" (p. 423). In contrast to this direct stimulation by the object, a "perception" is only "an indirect response to the physical stimulus", *e.g.* we hear a sound and "perceive" a motor car. The author expresses the difference between "perception" (which includes reasoning) and sensation as follows: "The chain of events is: stimulus, response of the sense-organ and sensory nerve, first cortical response which is sensation, second *cortical response* which is perception".

Therefore as reasoning is a form of "perception", Mr. Woodworth will have to show that reasoning is correctly described as a "response" of an area of the cerebral cortex when stimulated. Let us test his illustrations and examine his admissions. I submit that we shall find that he deserts his own teaching about reasoning, and that he does not explain the very facts which clamour for an explanation.

> "Suppose you need the hammer and go to the place where it is kept, only to find it gone. Now, if you proceed to look here and there, ransacking the house without any plan, that would be motor exploration. But, if, finding this trial and error procedure to be laborious and almost helpless, you sit down and think: 'Where can that hammer be? Probably where I used it last'; you may recall using it for a certain purpose, in a certain place, go there and find it. You have substituted mental exploration of the situation, for purely motor exploration, and saved time and effort. Such instances show the use of reasoning and the part it plays in behaviour. . . . It is an exploratory process, a searching for facts. In a way it is a trial and error process. If you don't ransack the house, at least you ransack your memory, in search for facts that will assist you" (p. 462).

(A) Of course we do all this. Mr. Woodworth truthfully describes this mental process—as far as he does describe it.

(B) But does his admission square with his theory that this "exploratory process" is a response to a stimulus of the brain cortex? I think not. Why? The hammerless man is "in search for facts that *will assist* him". Now how does he know what sort of facts will assist him? Does any stimulation of any particular nerve centre in the cortex make him ask the question: "Would the hammer be likely to be where I last used it?" What nerve centre when stimulated would have the extraordinary response consisting of the knowledge that the hammer is likely to be where the work was done. Is there any possible suggestion which would explain why this very remarkable nerve centre, rather than any of millions of others, should be stimulated? How, *e.g.,* can the general feeling of uneasiness, coupled with the desire for a hammer which I cannot find, so affect the cortex of my brain that the stimulus sets in motion just that one of the adjacent nerve centres which is accompanied by the "perception"—"tools are generally found close to where they were used"? To say that the stimulus-response hypothesis explains how the man "sets out in search of facts that will assist him", is futile.

(C) As already noted at some length, the perception of relations cannot be made to square with the stimulus-response theory. Now, in the illustration given above, there is prominently present the perception of a relation; for, the man sees the bearing of one fact on another—which is a very different thing from the perception of the facts themselves. The searcher sees the relation between the fact "my tools are generally left near where I work", and the fact "I was last working in the wash house". But to this relation there is not, and there could not be, any corresponding stimulus.

(D) The fact is that reasoning is much more fundamentally different from sense perception than Mr. Woodworth admits. It contains a rational element which is essentially different from anything in sense-perception; it is not, and could not be, what a sensation is, *viz.* a nervous tremor which becomes, or issues in, a conscious state. Of many reasons for this, one suffices: Any change in the cerebral cortex when I think: "Tools are likely to be found where they were last used", *follows* upon this thought—it does not precede and cause it. In the case of reasoning, thinking modifies the brain; brain modifications do not produce thought.

(E) Mr. Woodworth is so tied to his theory according to which reasoning is an "exploration" by a "sort of trial and error method" that he is almost unwilling to admit that a boy is reasoning when there is no "trial and error", *e.g.* when the school boy is endeavouring to understand the steps in a ready presented proof set out in his text book of geometry. But surely, only the rejection of the accepted meaning of words could prevent us from saying that the boy is using his reason. What else is he using? This much is certain: stimuli do *not* give him the required response. If they did, he would not have his difficulty in understanding the proof.

Inference:

> "Inference is the culmination of the whole exploratory process." "What (the successful reasoner) sees is not present to his senses at the moment, but he 'sees that something must be so'. This kind of perception may be called inference" (p. 465).

The author elucidates his view as follows:

> "Inference, typically, is a response to facts, and the response consists in perceiving a third fact that is bound up with the other two" (p. 466).

Again:

> "To sum up: The process of reasoning culminates in two facts being present as stimuli, and the response, called 'inference', consists in perceiving a third fact that is implicated in the two stimulus facts" (*ibid.*).

(A) This language could be interpreted so as to be acceptable to those who reject his theory and who hold to a radical difference between the nature of sensation (which is literally the response to a stimulus) and the nature of an inference which is not such a response. We willingly admit that in inference we see something which is implied in something else.

(B) As before, it can most tellingly be urged that *the implication itself* is not a stimulus—yet it is the implication itself which we know. The fact that since A is greater than B and B is greater than C, therefore A is greater than C—this latter "fact" which is implied in the other two, cannot in any sense be called a stimulus. It is not a sense object: it is not an interior condition of the body: it is not a movement of the brain cortex. And as these are the only stimuli that Mr. Woodworth allows, he is driven to accept the inference that his own theory of stimulus-response breaks down in the case of inference!

III

WILL AND DECISION.

Will (1):

By "will" Mr. Woodworth means the mental act, not the power or faculty of producing such an act. Hence he says little of the nature of such a power. The main points in his teaching are these: Will is a response to external obstruction or to internal conflict (p. 535). It is "action aimed at the accomplishment of foreseen results" (p. 527). He claims there is no need to begin by defining "will", on the score that in common speech it has various and conflicting meanings, *e.g.* "Since you urge me I *will* do this, though much against my *will*" (p. 523). Hence he tells us: "Will is not precisely a psychological term, anyway, but is a term of common speech which need *not* refer to any psychological unit" (p. 523). A typical act of will occurs when "we imagine some change to be produced in the existing situation and then proceed to put our imagination into effect: and this is a typically voluntary act" (p. 525).

(A) We are told that will "seems *not* to be any special kind of response". Now against this one might urge that as "will" is admitted to be "action aimed at the accomplishment of foreseen results", it is quite a special kind of response. Why? Because unlike ordinary responses, it does not follow directly upon the stimulus. What it does follow upon is the *foreseeing* of results—and a foresight of what will or could occur, cannot be a response to a stimulus in any of the author's three senses of that word.

(B) Again, we have no ground whatever for saying that any of the responses which we observe in animals, depend on foreseen results; whereas in man we have this dependence. Why do I deny that we have any right to say that an animal's attempt or 'will' to get out of a difficulty (*e.g.* a dog trying to open a gate) is an endeavour to accomplish a foreseen result? For this reason: In any such analysis of the content of the dog's mind, we are reading ours into his; we are arguing that because externally he *acts* as we should do, therefore he *thinks* as we should. But we have no right to explain the purposive behaviour of the animal in terms of our own purposive activity. We do not *e.g.* thus explain the marvellous purposive activity which joins the ends of a broken tibia or heals a gash in my finger. (I shall say more later on concerning Mr. Woodworth's invalid inference from animal behaviour.)

Now as there is no ground for saying that an animal "foresees a result to be accomplished", and as many of our willed responses have this foresight, there seems quite good reason for calling will a special kind of "response".

(C) "Aiming at the accomplishment of a foreseen result" implies seeing the relation between the result and the means required to attain it. But, as I have already twice noted at considerable length, the perception of a relation cannot be a response to a stimulus—for the reason that there is no stimulus.

The fact is that Mr. Woodworth has admitted that will contains a rational element; and has not noticed that this admission runs counter to his stimulus-response theory.

(2) Mr. Woodworth makes a suggestion as to the inner nature of "will". He cannot admit of a faculty, or distinct power of willing; for he regards "faculty psychology" as "rather useless, if not misleading" (p. 522); but he can tell us something about this power of willing, something about the origin of any "will":

> "The will to live is in a sense unconscious, since it is seldom present simply in that bald abstract form. But since life is activity, *any will to act is the will to live* in a special form, so that we may perfectly well say that the will to live is always conscious whenever there is any conscious impulse or purpose" (p. 565).

Let me take a case, which (A) contradicts this statement: which (B) cannot be explained on the stimulus-response theory: which (C) can be explained by his opponent who holds that will is quite other than Mr. Woodworth takes it to be. The test case which I suggest is a man's deliberate "will" to commit suicide:

(A) In the first place, the man wills *not* to live; obviously, then, this "will" cannot be *due to the will* to live. Here we have what seems an unanswerable objection to Mr. Woodworth's analysis of will.

(B) Secondly, the will to terminate his existence cannot be a response to any stimulus. For, even a thorough examination of animal behaviour, so much practised by Mr. Woodworth, gives no example of a stimulus which results in the animal's attempting to die; the response is always in the animal's attempting to live. Further, as far as I can see after repeatedly re-reading the author's treatment of "learning" and his chapter on "association", stimulus-response theory cannot furnish any explanation of the suicide's "response" to a difficult and humiliating situation.

(C) Those who oppose Mr. Woodworth and hold to an essential difference between "reasoning" and sense-perception —such psychologists can satisfactorily analyse the suicide's will to end his life: The man is faced with dishonour or physical pain, or some kind of mental torture. He envisages a painful existence and compares this with a painless non-existence (at least, what he thinks is such); or he compares the certainty of a painful existence with a future about which he asserts he knows little or nothing. The comparison is in favour of non-existence, and this he accordingly wills. In such a mental process we have nothing which is a response to stimuli. Instead, we have a use (albeit an incorrect one) of reason, which places before the mind an aspect under which the act of self-destruction appears as a good. There follows an appetition, or tendency or "will" for this good. This act of willing is therefore rational, in so far as it is directed by reason. Now, I submit that this is an explanation which fits the facts, whereas the stimulus-response theory fails to do so.

(3) Mr. Woodworth suggests: "It does seem that will . . . is almost the same thing as the instinct of self-assertion" (p. 536). Two obvious comments suggest themselves:

(A) In many cases will does imply self-assertiveness, in the sense that the continuance of the self is implicitly aimed at. But the converse is not the case, *viz.*, that self-assertiveness implies will. For, the "self-assertiveness" of the acorn in the ground is not usually called will. Hence we should not make the general statement that will is almost the same thing as self-assertiveness.

(B) The case of deliberate suicide is a crucial test which proves the author's view to be untenable.

Decision:

(1) On the vitally important question whether all man's actions are inevitable and could not but be what they are, given all the present circumstances—*i.e.* on the important question whether man has the power of free will, Mr. Woodworth seems at least undecided, if not inconsistent. On p. 18 we read: "For the psychologist conduct is a matter of cause and effect, of natural law". And again (p. 68):

> "Stimulus-response psychology is solid, and practical as well; for it can establish the laws of reaction, so as to predict what response will be made to a given stimulus, and what stimulus can be depended on to arouse a desired response. It furnishes the 'Knowledge which is Power'."

Now it may be unfair to regard these statements as intended to apply to *all* man's conduct, including *all* his decisions; but at least "prima facie" this seems to be the meaning. It is at least implied that the author is a rigid determinist; for he seems to hold that given a man's education to have been what it was, and given the whole content of his mind to be what it is, then when faced with the temptation to steal £5, his "response" is as much a natural law as is the velocity of a falling stone at any point in its course.

If this is the author's meaning, then, as I have elsewhere argued, individual responsibility goes by the board: remorse and repentance have no meaning: moral obligation is a word which can find no place in ethics. Why? Because of every action (anger, murder, theft, incest, cruelty) we can and must say: "It had to be what it was; it could not have been otherwise." If this is so, then we cannot rationally hold that the man *should have* done otherwise since he *could* not.[1]

(2) When Mr. Woodworth is analysing a typical case of moral choice, he stops short precisely there where a psychologist should press forward with his inquiries: "Then there is the case where we 'see the better but follow the worse', or are in danger of so doing."

He asks:

> "The lower motive being the stronger, how can it ever be that the higher motive gets the decision?"

He replies:

> "Well, the fight is not just a contest between these two. Other motives are drawn into the fray, the whole man is drawn in, and it is a question which *side* is the stronger. Fear of ridicule or criticism, sense of duty, self respect, ambition, ideals of oneself, concern for the welfare of another person, loyalty to a social group, may be ranged on the side of the weaker motive and give it the advantage over the stronger" (p. 533).

The tenor of this passage suggests all too obviously that the decision just *happens* to occur *in* me: motives *get* ranged on one side or the other: I happen to have an ideal and to think of it at the moment, and it thus gets ranged on the side of the nobler decision. If this is the author's meaning, then he robs the man of all obligation and responsibility. For, the ensuing decision could not but have been what it was. If on the other hand, Mr. Woodworth does not imply this, but means that it rests with the man which motives will be made the stronger by being exclusively attended to, then the analysis stops short just when the interesting question looms up before us—What determines whether one set of motives rather than another is to be the object of the man's attentive consideration? In either case, then, there is little, if any, value in Mr. Woodworth's analysis.

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[1] I am aware that this argument would have no force against thinkers of the school of McDougall—at least if the implication of their teaching on punishment is admitted by them.

(3) It is in his discussion of "decision" that Mr. Woodworth very obviously uses an invalid principle of observation. He writes:

> "The lowest animals have few reaction tendencies, and being responsive to only a narrow environment, show little signs of internal *conflict*" (p. 529).
>
> "In the behaviour of higher animals we sometimes detect signs of a longer persistent *conflict*, as between curiosity and fear, when a wild creature seems poised between his inclination to approach and examine a strange object, and his inclination to run away, veering now towards the one and now towards the other, and unable, as it seems, to reach a *decision*" (p. 529).

This is the principle on which the argument is based: Because the external behaviour of an animal is similar to that of a human being in analogous circumstances, therefore its mind is acting in the same way as the man's. We might just as well argue that when the magnet brings a piece of iron to itself, it effects this in the same way as does my hand, *viz.*, by physical contact with the iron. It is idle pretence to say that we can observe what is the content of the animal's mind. What grounds *e.g.* have we for saying that when a cat is faced with a large and a small saucer of milk, it "decides" which to take, in the same way as a child "decides" between a large and a small banana, or in the same way as a man "decides" between honest and dishonest conduct? If Mr. Woodworth applied his stimulus-response theory only to animals—where, alone, it is applicable, because here we know only the responses and the stimuli—and if he did not arbitrarily interpret the animal's behaviour in terms of human consciousness, he could not have fallen into the logical error of saying that animals form "decisions" or have "internal conflicts".

IV

SOME FURTHER TESTS.

Let us examine the author's application of his theory to "invention", "manipulation" and "memory". It will again appear that his theory does not fit the facts, and that its speciousness is due to using language that glosses over and takes no account of the characteristic feature of these activities.

Invention:

> "Typically, the preliminary stage consists in recall; and association by similarity, bringing together materials from different past experiences, is very important as a preliminary to invention. Facts recalled from different contexts are thus brought together, and *invention consists in a response to such novel combination of facts.*

The two steps in invention are, first, getting a combination of stimuli; and second, *responding to the combination*" (p. 519).

(A) Admittedly all this does occur in invention.

(B) But ought we to remain satisfied with the explanation that invention is a response to a combination of stimuli? I submit that we ought not. Because in the first place, it seems impossible to point out any stimulus[1] which brings about this alleged response. The inventor, *e.g.,* of the "differential" in a motor car's rear axle of course did gather together a number of facts from past experience. But neither singly nor collectively could these have produced such a stimulus in the brain cortex as to evoke the concept of an entirely new and strange combination of cogs which would allow the two wheels to change their relative rates of revolution exactly in the right proportion when the car is rounding a sharp corner. The sudden arrival of such a stimulus would be nearly as strange as the arrival of the leading article of tomorrow morning's paper, if ten thousand letters were shaken together in a bucket and then tossed out on to the floor.

(C) The fact is that the word "response" glosses over the important element of reasoning or inference, found in such an invention. The inventor uses all his available past knowledge of facts to frame a contrivance which is related to an end. His genius consists in seeing many different ways in which objects *could be related,* and in picking upon that particular relation which *might* serve the purpose. Now, to see *possible* combinations, and *possible* relations, and *possible* suitabilities —this is something more than to "respond" to a stimulus. For, what is not actual, but only possible, cannot be a stimulus. Invention implies a rational activity of which Mr. Woodworth takes no account.

Manipulation:

In Mr. Woodworth's terminology, "manipulation" is almost a synonym for "inventive activity" (p. 481); it is a sort of "exploration". He illustrated it as follows:

> "The baby picks up his new toy, turns it about and examines it on all sides, shakes it and is pleased if it makes a noise, drops it and is pleased with its bang on the floor. This is manipulation certainly, but it is also a way of exploring the properties of the toy" (pp. 481-2).

Again, we have more developed forms of manipulation, such as:

> "Taking things apart and putting them together, assembling dolls and toy animals into 'families' and 'parties'" (p. 482).

A more perfected form of manipulation is make-believe.

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[1] We must constantly bear in mind the author's three kinds of stimuli.

> "The little boy puts together a row of blocks and pushes it along the floor asserting that it is a train of cars. . . . This might be spoken of as 'manipulating things *according to the meaning* attached to them'" (p. 482).

The manipulation of make-believe later evolves into story-telling. The child "talks about persons and things and makes them perform in his story".

The reader's careful attention is called to the explicit admission in the passage which follows the one last cited:

> "In such ways as we have been describing, the little child shows 'imagination' or mental manipulation. In story telling the objects manipulated are simply *thought of*; in make believe though there is actual motor manipulation of present objects, the attached *meanings* are the important matter; and in construction there is apt to be a *plan* in mind in advance of the motor manipulation, as when you look at the furniture in a room and consider possible rearrangements" (p. 483).

(A) The application of the one term "manipulation", without any specific difference, to various activities at least suggests that these are all specifically the same, and that they vary only in accidentals of quality and quantity. For example, if we are told that a terrier pup playfully "manipulates" a kitten which he sees for the first time: and that Edison "manipulates" his materials, we are apt to think that there is no radical difference between the two activities. We are likely to conclude that the terrier's "manipulation" could (and possibly did) evolve into that of Edison. I submit that this use of language is unnecessarily vague and is also misleading.

(B) The sentences containing the words which I have italicised in the last quotation, are tantamount to an admission on their author's part that his stimulus-response psychology cannot explain the facts. For, even though at the cost of repetition, let us recall accurately what he means by a stimulus. It is either an external force or motion (*e.g.* light or sound): or internal changes in the bodily state, which act on the sensory nerves (*e.g.* hunger, fatigue): or "central stimuli"—here one nerve centre directly stimulates another, and this latter stimulation evokes a new thought or new emotion or a "will" *et cetera*. The important point in the theory is that a mental activity does not, and cannot, come into being except as the response to some one of these three stimuli; it is evoked by the stimulus. The theory does not allow us to conceive of the thinking principle (*i.e.* the subject which thinks, or the mind—whatever we take its nature to be) as *itself* originating any thought, or any emotion, or any creative or directive energy; there is no mental activity which is not a "response"—and a response to one of the three stimuli. True, the author does not explicitly make this statement; but it is the very core of his teaching; and the main quarrel I have with him is his seeming forgetfulness of what is implied in his theory.

Now I urge that in the paragraph last quoted, Mr. Woodworth rejects his own teaching. How so? For these three reasons:

(i) "In story telling, the objects manipulated are simply *thought of*." For example, the child amuses himself and us by "rushing into the house with a harrowing tale of being pursued by a hippopotamus in the woods". One immediately asks: What is the stimulus whose response is an excited rushing indoors: and a vivid mental picture of a hungry hippo chasing a small boy? If Mr. Woodworth really means what his words imply, then it seems that we have a most remarkable response without any stimulus; for as the result of their proper stimuli, certain thoughts (*e.g.* of the hippopotamus, the woods, the house, *et cetera*) can *merely be* in the mind—*"they are simply thought of."* Thus there is nothing whatever to account for the most interesting activity of *enjoying* oneself by *imagining* a situation which is *known* to be unreal and by *stirring* up *excitement* which is *known* to be groundless, and by *informing* other people of what both they and the informant know to be false. This is indeed a most complicated response, and contains a great variety of elements. It cannot be the response to stimuli. Why not? Because these account merely for the *presence* of the three thoughts *in* the mind. The word "response" glosses over, then, the fact that creative and directive energy is exercised by the mind and is not evoked by stimuli. The cause of Mr. Woodworth's difficulty is, I suspect, his dislike for the notion of a *mind*; he knows only man in the concrete; he will not discuss metaphysics nor deduce the nature of the mind from the way in which it acts; such examination of the psyche is banned from psychology.

(ii) "In make believe . . . the attached *meanings* are the important matter." I have already argued at length that the perception of meaning cannot be explained in terms of stimulus-response. For example, when, in one case, a lion and a stout iron fence in front of him is presented; and when, in the other case, the lion is presented without the iron fence, the stimuli received by the eye are not so very different—one is merely more complex than the other. This being so, how can Mr. Woodworth account for the response being so different in the two cases? Why in the one instance should I respond by *knowing that I ought to* run for my life; and in the other by knowing that I ought not? None of Mr. Woodworth's three kinds of stimuli can offer the slightest explanation of this.

They might indeed possibly explain the difference in outward behaviour—though even this could be questioned—but they cannot explain the awareness of an obligation in the one case and of none in the other.

(iii) "In construction there is apt to be *a plan in mind* in advance of the motor manipulation." When discussing "invention" I laboured the point that we have here evidence of creative activity which implies the seeing not only of objects (sources of stimuli), but of relations, and I urged that this latter knowledge could not be a response, on the score that there was no stimulus. Now, all this equally applies to holding before the mind a plan which I am to follow in order to secure a desired result; for here there is creative activity and also the perception of relation. By admitting a "plan in advance", then, Mr. Woodworth can be accused of rejecting his own theory of stimulus-response.

Memory:

Memory implies more than the mere retention and recall of past experiences. For, its most conspicuous feature is the *recognition* of a present as being the same as a past. Now this feature of memory is quite inaccurately described by Mr. Woodworth. "Recognition", he tells us, "is a form of *learned response,* depending on previous reaction to the object recognised" (p. 358). He takes notice only of the external actions and of the internal emotions; he passes by, without any comment, the peculiarly distinctive feature of memory, viz., recognition; and he fails to note that this mental activity necessarily implies a *comparison* of the present with the past. About the fact that in recognising things and persons and places we do thus compare, there cannot be any doubt, and it would be wasting time to suggest illustrations.

Now with regard to an act which compares two things I note (A): it emphatically is not a response to either or to both stimuli. Why not? Because the stimuli could account for only the perception of the objects—not for an entirely new and different activity; (B), once again there is a perception of a relation (here of similarity) between the past and the present; and, as already noted, such a perception is not evoked by any of the three stimuli listed out by Mr. Woodworth.

Imagination:

> "Imagination is like reasoning in being a mental reaction . . . reasoning consists in seeing relationships that exist between facts, and imagination in putting facts into new relationships" (p. 484).

I need not repeat my criticism. It is sufficient to note that the author openly admits the perception of relations, and that he implies that we can exercise a creative activity. In each of these two admissions he is inconsistent with his own theory.

V

INACCURATE ANALYSES.

There are several instances where Mr. Woodworth's analysis of mental activity seems inaccurate and misleading. The following instances will serve to substantiate this charge. *Learning*:

(1) Watching the movements of a hungry caged cat in its endeavour to find a way out, the author notes that the animal makes a great number of "*random* movements"—it scratches, pulls, jumps, lies prone and stares about, pushes, crouches, walks, *et cetera*. One of these random movements undoes the door of the cage. After several repetitions of this same experience, the animal "learns" how to undo the door. The learning, we are told, is by "trial and error".

It should be carefully noted that the "trial and error" as exemplified in the cat, means a number of *random* movements. The animal, as it were, explores in all directions: every possible movement of which he is capable, is executed and in an utterly chance fashion. This, I repeat, is what Mr. Woodworth means by "trial and error".

Now he is seriously inaccurate when he tells us that it is by "trial and error" that a child very slowly learns to pronounce words: or an amateur to hit a nail fair and square on the head: or an adult to open a patent cigarette lighter. It would be difficult to parallel the inaccuracy of the statement that such instances of learning "proceed by trial and error". Why is it inaccurate? Because if there is one thing about which we refuse to doubt, it is that these actions are not random—they are all consciously purposive: they are all directed: they are all, or many of them, done with the object of testing out a suggestion. There is indeed trial and there is error. But to say that our carefully reasoned out attempts are similar to the cat's "trials" and that our initial failures are like the cat's "errors" is simply to contradict the verdict of our eyes concerning the cat, and also the verdict of introspection concerning ourselves. Cage a hungry man and a hungry cat, and watch the radical difference in their attempts to escape. There is no "random" movement in the one case; there is in the other.

Mr. Woodworth's chapter on "learning" is replete with the inaccurate use of this allegedly descriptive phrase "Trial and error". When speaking of man or the "higher animals", he uses it in the sense of "unsuccessful trial". He ought to use it only in the sense of "random movements, some of which fail and some succeed".

(2) Here is another instance of faulty analysis: The author explains how by "the linkage of a name to an object" a child learns the names of things.

> "A child who can imitate simple words that he hears is shown a penny, and the word 'penny' is spoken to him. To this combination of stimuli he responds by saying the word. This is primarily a response to the auditory stimulus . . . which determined the (vocal) response, and attracted the visual stimulus into this particular channel of saying 'penny'. The linkage from the sight of the penny to the saying of this word, being thus strengthened by exercise, the seen penny gives the right vocal response, without any auditory stimulus to assist" (p. 403).

The analysis would be accurate, and possibly complete, in the case of a parrot who is "taught" to call out "postman" when that worthy appears at the front gate. It would be accurate of the child only if we presumed that he learns merely to make certain vocal noises when he sees a penny. If this is the correct analysis of our "learning the names of things", then names would never be learned; we should learn merely sounds, not *signs*. The author's paragraph is entitled "Learning the names of things"; its caption should be replaced by "Learning to make sounds when things are seen".

> (3) "Combination of reflex movements into larger co-ordinations is alleged to explain the learning of new movements, *e.g.* one hand grips an object, while the other pulls, pushes or strikes it. Or both hands grasp the object, but in different ways, as in handling an axe or a shovel" (p. 299).

This is unconvincing. Why? Because if the learning how to bend a stick consists in the combining of reflex movements, we shall have to say that the pushing with one hand and the pulling with the other are each reflexes. But if this is so, then we have two quite opposite reflexes when there is but the one stimulus—which Mr. Woodworth could hardly admit.

For a similar reason, in the very distinctive swing of the bushman's axe (with its sudden change of action at the top of the stroke allowing the axe to slide through one hand and not the other, while the drive forward of the axe becomes a pull downward), the different movements cannot *all* be reflexes which are responses to the *one* stimulus. Admittedly, the movements are for the most part automatic. But they become so through practice; and, learning to swing an axe deftly with the minimum of energy and the maximum of efficiency, cannot be described as a combining of reflex actions. Put an axe into the hands of a city boy, and supply all the stimuli required —among his "reflexes" you will not get any which by combining would produce the typical bushman's stroke with its typical bodily movements.

The same very obvious criticism applies to Mr. Woodworth's example of dancing as a "serial co-ordination": "*Instinctive* movements become linked together"—as in dancing, *et cetera.* I wonder what movements of the tap-dancer are *instinctive?* or of the ballet-dancer? or even of one enjoying the good old barn-dance?

(4) Here is an instance of inaccuracy in devising and in interpreting an experiment: In the power of "learning" a maze, a comparison is made between the rat, the child and the adult man (cf. pp. 314 and 315). The details of this serious and consequential experiment are set out as follows:

> "The subjects tested consisted of 23 rats, five children varying in age from eight to thirteen, and four graduate students of psychology. The human maze was *much larger* than that used by the rats, but *roughly* about the same in complexity. Since rats are known to make little use of their eyes in learning a maze, the human subjects were blindfolded."

(A) With what scientific accuracy did the experimenters determine the relative sizes of the maze for the rat and the graduate students? And how did they accurately fix the relative degree of complexity? There is so very little chance of the "subjects" being faced with an identical problem, that the experiment seems utterly worthless.

(B) The rats were *allowed the full use* of all their natural powers of learning; whereas the graduated students were *deprived of any use* of the most natural of their powers of learning—they were blindfolded! The experiment is about as scientifically accurate as would be a comparison between the human and the canine powers of swimming, where the dogs could swim in the way most natural for them, while the men could not use the "trudgeon" or the "crawl", but were allowed only to dog-paddle.

Such scientifically inaccurate "experiments" are not uncommon in Mr. Woodworth's Psychology.

VI

FAULTY LOGIC.

(1) In interpreting the behaviour of animals and of infants it can be urged that Mr. Woodworth relies on a principle which is false. His interpretation of animal conduct always involves the assumption that similar results must have the same explanation: that when the effects (at least as externally observed) are the same, the causes must be the same.

Almost any one of the illustrations of animal behaviour shows that the author's argument does depend on this principle. For example, in the case of the white rat that "learns" to act by signals (cf. pp. 306 and 436) we are told: "He looked all round, seeking, we may say, a stimulus to guide him." That is to say, if we acted in a similar manner it would be due to our seeking for something to guide us; and because the rat acts in this way, it must be due to his thinking as we do.

And here is another illustration: Of the rat in the maze we read: "He *knows* . . . that he is in a strange place, *to be explored with caution* on the odd chance that it may contain something eatable, or something dangerous" (p. 313). Here the implied argument is the same. In similar circumstances our conduct would be due to our knowing that the place was strange and possibly dangerous and needing caution; but the rat's conduct is like ours; therefore it must be explained by a similar knowledge. Examples could be multiplied indefinitely.

Now the principle underlying such an argument is manifestly false. As already suggested, one might just as well argue that because the magnet brings the iron to itself as truly as we do, therefore the magnet does it in the same way as we do, *viz.*, by making physical contact with the iron.

Or we might show the fallacy of the principle, by a "reductio ad absurdum": When soldiers on the march are faced with a shell hole in the road, they fill in the hole and repair the surface of the road. And when the blood corpuscles in my thumb are faced with a deep gash in the flesh, they slowly but in the most scientific manner, fill up the gap with new cells, then repair the surface by new layers of skin. But as the action of the soldiers is to be explained by their knowing what is to be done, so also in the case of my blood—it knows what is to be done.[1]

(2) Here is another false principle: "In saying that two or more stimuli arouse a single response, we imply that there is *already* some rudimentary linkage between each stimulus and the common response" (p. 399).

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[1] In fairness to Mr. Woodworth it must be noted that he explicitly denies that the caged cat uses reasoning in its endeavour to escape. "There is no evidence that the cat reasons his way out of the cage. His behaviour is impulsive, not deliberate. There is not even any evidence that the cat clearly observes how he gets out" (p. 310). This being so, one wonders how the cat's behaviour can shed any light on that of the human being who learns to solve a puzzle. For granting that the human being uses reason and that the cat does not, the way in which they both learn will be entirely different. This is one of several inconsistencies that strike me when carefully reading Mr. Woodworth's treatise.

I suggest the comment that two causes may in combination produce a result with which neither of the causes, when taken singly, is linked even in a rudimentary way. For example, a draught of water saves my life when I am dying of thirst; but separate draughts of oxygen and hydrogen are utterly useless—neither the oxygen by itself nor the hydrogen is "linked in a rudimentary way" with the quenching of my thirst.

(3) Here is still another illustration of faulty reasoning: The argument in favour of calling "perception" a "cortical response" is that there cannot be "perception" if the cortical area is not functioning.[1] The reasoning is obviously invalid, as examples show: I cannot see a man three miles away, unless I use my telescope. This does not show that my sight is the response of the telescope. Or again, I can write, only if the ink in my fountain pen is not muddy; yet the clarity of the ink is not the cause of my writing.

The author's argument confuses a cause and a condition, and also confuses an agent and an instrument. For Mr. Woodworth argues that because I cannot "perceive" unless a certain area of the brain cortex is in order, the cortical area itself evokes the "response".

(4) Let me expand an argument suggested in the second last footnote. Mr. Woodworth tells us that the hungry cat, learning to escape from its cage, does not reason his way out; its behaviour is not deliberate, but is purely impulsive (p. 310). Now I take this as tantamount to an admission that the cat *cannot* reason—for it does not do so, even to save its life: and also as tantamount to the admission that the same is true of the rat in the maze. Now because the "learning" of these animals is allegedly due to a "substitute stimulus", the author concludes that a child "learns" the names of things in the same way (p. 403).

This seems most inconsistent. Why? Because the author is entitled to draw a parallel between the "learning" of a cat and of a child, only if he makes them equal as regards the power of reasoning. If they both use reason, then Mr. Woodworth goes back upon his admission about the cat's absence of reason: if neither uses reason, then he gratuitously assumes that the child does not act in the way that is most natural for him: if one uses reason and the other does not, then we cannot possibly conclude anything concerning the way one learns, from the way the other learns. The fact of their "behaviour" being much alike gives us absolutely no warrant for any such conclusion as that drawn by Mr. Woodworth.

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[1] Cf. p. 423. "The cortical regions adjacent to the sensory areas are necessary for perception; if they are destroyed, the individual may still see, but not know the object seen; or may still hear, but not recognise the words or the tunes he hears." My criticism above is subject to my being correct in taking this sentence as a proof for the "cortical stimulus" being accountable for the "response" of perception. It is just possible that the author is merely *stating* some facts that have been discovered.

Conclusion.

While I admit that there is much that is valuable in Mr. Woodworth's Psychology, I have tendered evidence to show that there is very little, if any, value in his theory of stimulus-response. There were other points on which I could have offered further criticism; but I have noted a number sufficient to stimulate into defending him those who disagree with my findings.

MODALITY.

By P. H. PARTRIDGE.

I WISH, to begin with, to consider two or three doubtful points in Johnson's chapter on modality in *Logic,* Part I, to see what conclusions can be reached as regards the nature of the modal adjectives. So far as the term "necessary" is concerned, if it means merely necessitated, we must agree, of course, that everything is necessary—assuming that every event has a cause. Thus no one could deny the existence of what Leibniz calls hypothetical or physical necessity; and, again, in the remarks on the Fourth Antinomy, Kant makes a distinction between intellectual and empirical contingency, which would imply, presumably, a corresponding distinction between intellectual and empirical necessity. The necessity here admitted would, of course, include contingency; what is necessitated is contingent. But since, in rationalistic theories, a type of necessity opposed to contingency is postulated, it is obvious that necessity is not simply necessitation. This absolute, or metaphysical, necessity is opposed also to another type of necessity we might freely recognise, *viz.,* the sense in which "A is necessarily B" indicates merely that "All A are B". On a theory of metaphysical necessity, we should be required to distinguish between the three propositions: All A are B, all A are possibly B, all A are necessarily B; and it is the object of this paper to discover whether such a distinction can, in fact, be made. A point that may be noted in passing is that if, in this way, we contend that "A is possibly B" indicates that some A are B, we are not implying at all that there is an A that is possibly B; any A is either B or not B, and, further, if we say that whatever is, is necessitated, its being B, or not B, will be necessary. Thus, the possibility of an A's being B refers, so to speak, to what we might discover, but we should have to say that whether that A is B or not B is completely determined.

So far, then, we can recognise modal distinctions as characterising the attitude of the knower. If the possibility of a proposition means that we do not disbelieve it (although we do not confidently affirm it), the necessity of a proposition will mean simply that we do believe it. A is necessarily B; *i.e.,* all A are B—we are prepared to assert that any A is B.

Of course, necessity in this sense is to be distinguished from necessity in the sense of being necessitated; we might express the position by saying that while every proposition is necessitated, there are some propositions we are neither prepared to assert nor prepared to deny. The interpretation of necessity as necessitation, as we saw, eliminates contingency as a peculiar mode, and also possibility (though we must return to this point later); and taking necessity and possibility to refer to our attitude towards a proposition, contingency as a peculiar mode is again dispensed with, since we must either affirm (or deny) a proposition, or take up the position that we do not know whether it is true or false (possibility).

Now, according to this second statement of necessity, to say that we believe a proposition to be necessary will mean only that we believe it to be true, that we assert it. "A is B", "A is truly B", "A is necessarily B" will be all the same assertion; the second two propositions add nothing whatever to the first. Similarly, "A is possibly B" will mean that "A is B" is a proposition we neither assert nor deny. This condition of our minds is a real fact, not a possible fact; and when it is said, as, for example, of any significant assertion, that it can be possibly asserted or denied, all that can be meant is that, lacking information, we should not know whether to assert or deny it. But nothing can be meant by saying that the proposition can be possibly true, and possibly false; it is either true or false, and I should add, necessarily so. This is what Johnson calls epistemic modality, and the question that arises is whether he is justified in distinguishing from it "formal" and "constitutive" modality; or, in fact, whether any other account can be given of it, on any view.

"It has sometimes been said", Johnson[1] says, "that *all* propositions should be conceived as necessary; in the sense that the asserter of a proposition represents to himself an objective ground of reference to which he submits, and which restrains the free exercise of his will in the act of judgment. This contention is indisputable." And, later on, he defines necessity (in this sense) as "the compulsion exercised by the object or real upon the thinker". Now, the point in this statement of the position is that it implies, not that something is asserted to be necessary, but that something is necessarily asserted, and if, then, we treat necessity as the necessitation of the thinker by certain other conditions, there is no reason to retain any further the distinction made at the beginning between a thing as necessitated and a proposition as necessary in the sense of being definitely affirmed. We may say, then, that whatever proposition we accept, we accept as necessary, or necessarily accept it; and if, in this sense, we are to preserve the contingent, it must be maintained that some propositions are accepted, though there is no "compulsion" upon us to accept them. But before dealing with this point, we may consider possibility in this way; and we should have to say, I think, that with a proposition which we neither affirm nor deny, there is no "compulsion" acting upon us, and hence no relation of belief between us and the proposition. But this implies that the necessitation being absent, it is impossible for us either to affirm or deny it; that it becomes possible for us to do one or the other only when that "compulsion" does occur, and our affirming then becomes necessary. The conclusion would be, then, that there is no "possibility" of our affirming or denying any proposition; we actually do not affirm it or deny it, but what we shall do, or what we finally do, is as fully necessitated as anything can be. And, so far as we do not go beyond the definition of a necessary proposition as what we necessarily affirm, it cannot be maintained that it is only true propositions that are necessary in this way; any proposition, true or false, so long as someone is necessitated to accept it, is a necessary proposition. This point is important, because when we come to the theory of "formal" necessity we shall find that what is implied is that it is the "necessity" of the propositions that necessitates their being believed.

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[1] *Logic,* Part I, p. 59.

To return to the question of contingency, the point is whether there are any propositions that we do accept or reject, though there is no "compulsion" upon us to do either. Johnson himself in the passage quoted, contends that every asserted proposition is necessary in this way, and we may take it that this is the case. Thus, so far as "epistemic" necessity is concerned, there will be no place either for contingency or possibility, only what is asserted can possibly be asserted, and all that is asserted is necessarily asserted. Epistemic possibility, Johnson defines as "what is not known to be false; *i.e.*, what does not contradict the necessary in the first sense ['epistemic'] applicable to all assertions". But a proposition must either be asserted, or not asserted; if it is asserted it is necessary, if it is not asserted, this means that it is impossible for us to assert it, *i.e.*, on this showing, it itself is impossible. On a theory of epistemic necessity such as this, there is no question of non-contradiction.

The necessity that belongs to propositions necessarily asserted is a contingent necessity: *i.e.*, their being asserted is contingent upon certain conditions. So far, then, nothing has been said to show the existence of any absolute or metaphysical necessity of the sort that a rationalistic theory stands for. We may say that a theory of absolute necessity is one that contends that certain things (or propositions) are self-necessitated, as with the necessary being, and the necessary truths to deny which is to assert them. Now, it is clear that most theories of absolute necessity that have been advanced are theories of what Johnson calls "formal" necessity, and that most thinkers would not accept the type of necessity that he calls "constitutive". For starting from the distinction that he makes between the formally (*i.e.,* the rationally) certified, and the experientially certified, "constitutive" necessity falls within the field of the experiential. This, of course, is opposed to Kant's view that "necessity and strict universality are infallible tests for distinguishing pure from empirical knowledge"; and, again, Leibniz's general position would be that the necessity of things is rational, and it is only the contingency, or existence of things that experience can inform us of. We may take it that the theory of absolute necessity is the one that Johnson describes as formal necessity.

Now, according to such a view, an attempt is made to argue away any possible distinction between a proposition's being known as necessary and its being necessarily known. The chief difficulty as regards such a theory is to distinguish it from epistemic necessity; but we may say, to begin with, that it is the proposition's being known that constitutes its necessity in the one case, and its being necessarily known that constitutes its necessity, in the other. Thus, the criterion of a necessary truth frequently given, *viz.,* the self-contradiction of its contradictory, amounts to the assertion that a necessary proposition is one that must be asserted. For Kant also, it is the necessity of our affirming such a proposition that constitutes its necessity: "The apodeictical proposition cogitates the assertorical as determined by these very laws of the understanding, consequently as affirming *a priori,* and in this manner it expresses logical necessity." And there is the further point that with this theory, we have an attempt at the discovery of the self-necessitated; since the proposition is determined by the "laws of the understanding", and the laws of the understanding are determined to determine it by the nature of the proposition. As we shall see, unless the relation of the faculty of cognition to a proposition is determined by something in the character of the proposition itself, no theory of absolute necessity is being presented at all; but apart from any argument on the question, Kant himself shows, in distinguishing between the problematical, the assertorical and the apodeictic, that it is the character of the proposition that determines the relation, *e.g.*, "The assertorical speaks of logical reality or truth; as, for example, in a hypothetical syllogism, the antecedent presents itself in a problematical form in the major, and in an assertorical form in the minor, and it shows that the proposition is *in harmony with the laws of the understanding.*"

If, then, we can distinguish propositions in harmony with "the laws of the understanding", from those determined by "the laws of the understanding", we cannot say that the relation to the understanding is determined by the understanding itself, in a purely arbitrary manner. In the first place, it would be impossible to distinguish a proposition "determined" from one merely "in harmony with"; to distinguish, *i.e.*, the necessary from the possible. Again, if the laws of the understanding are self-determined, if their determination and non-determination of propositions is purely arbitrary, there would be nothing to show that a necessary proposition will ever in fact be true. And it can be argued, if the relation between the proposition and the "laws of the understanding" is self-determined, then we have no more than "epistemic" necessity; it may not follow that any proposition will, as a matter of fact, be determined by the laws of the understanding, but any proposition asserted will be determined, or its assertion will be determined, by some cognitive factor, and there is no reason for accepting the one sort of proposition as necessary while denying the necessity of the other.

It may be said, then, that the relation of the proposition to the understanding will be determined by some characteristic of the proposition. This contradicts Kant's statement; that "the categories of modality possess this peculiarity, that they do not in the least determine the object, or enlarge the conception to which they are annexed as predicates, but only express its relation to the faculty of cognition". Now, I have tried to show that if they do only express the relation of the object, or conception, to the faculty of cognition, this gives no special certainty of truth to the proposition accepted as necessary. We are, we may admit, determined to accept a certain proposition; but we are determined to accept any proposition, true or false, that we do as a matter of fact accept, and the fact that in the one case, we may be self-determined, and in the other not, does not affect the matter at all. A "formal" necessity of this sort is as contingent a necessity as is "epistemic" necessity· and if "formal" necessity is to involve absolute necessity as a character of propositions, it can only be if there is some character of the proposition that determines the relation to the cognitive faculty. And, whether or not the position can be sustained, this implies that the categories of modality do determine the object or the proposition.

That "formal" modality cannot be regarded as something appended to propositions without affecting their meaning can best be demonstrated by criticising an argument of Johnson's. He is discussing the manner in which a modal proposition can be contradicted, and attempts a distinction between the contradictory of a proposition, and the contradictory of a secondary proposition—*i.e.,* one attributing modality to a primary. Now, he argues,[1] "in doubting or contradicting a *secondary* proposition, we use the opposite or contrary modal predicate; but in denying the *primary* proposition, we should attach the same modal adjective to the proposition and to its contradictory". We have, then, the two cases: "A is B is necessary", will be denied, so far as its modality is concerned, by the assertion "A is B is contingent"; but to deny the *primary* proposition "A is B", if it is a necessary proposition, we must assert "A is not B" as a necessary proposition, and this means that we are denying "A is B is necessary" by "A is not B is necessary". It can easily be shown that we cannot deny the parts of such a proposition independently, without making nonsense of the whole theory of "formal" modality; and the fact that it is impossible to contradict a proposition in two different ways shows that the modality of the proposition must be taken as an essential part of the meaning of the proposition, whatever difficulties may arise from this.

To take first the contradictory of the primary proposition, the modality being unaffected, Johnson argues that if "It is raining now" is contingent, then "It is not raining now" will also be contingent. The conclusion does not follow; if the first proposition is a contingent truth, the second proposition will simply be false. Similarly if "A is B" is a necessary truth, then "A is not B" is necessarily false; on the other hand, if we assert "A is not B", then "A is B" will be false; *i.e.,* it will simply not be necessary, and there is nothing to show, therefore, whether its contradictory would be necessary or contingent. Here, of course, Johnson is assuming that the modality of the proposition is independent, or can be treated independently, of its content—or, rather, of its truth or falsity. But we cannot escape the conclusion that if we can recognise the necessity of the proposition independently of its truth, the necessary proposition will be one with a possible contradictory, and the conception of necessity will disappear.

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[1] *Logic,* Part I, p. 54.

Much the same argument may be used in the second case, where we are supposed to be able to doubt the modality of a proposition, its truth being unaffected by the conclusion reached. In terms of the theory of "formal" necessity, this will imply that we can recognise the truth or falsity of a proposition independently of our recognition of the way in which it is "certified", or "related to the faculty of cognition"; that we can be correct about the former, though mistaken about the latter. The issue, in this case, will be whether the proposition is "formally", or "experientially", known. But, in the first place, if we treat this as a possible issue; *i.e.*, if we admit the possibility of a contradictory to the proposition, "A is B is formally certified", we admit, by implication, the possibility of a contradictory of "A is B". No matter, then, whether we recognise the proposition "formally" or "experientially", we can never recognise it as necessary, since this is precluded by the existence of a possible contradictory. Or, in the second place, if the way in which any proposition is certified is a possible issue, it can be settled only by experience, and, however a necessary proposition is known, its modality is always given empirically. But, according to Kant, "the conception of necessity . . . cannot be given by experience".

Thus, to preserve a theory of "formal" necessity, we should be compelled to argue, I think, that if a proposition is necessary, it must be known as "formally" known, or that the way in which it is known must be part of the content of the proposition. The alternative to this would be to contend that the manner of its being known, or its "relation to the faculty of cognition" has nothing at all to do with the modality of the proposition, and in this way it might be possible to show that necessary truths can be empirically known. But that is a point that does not at the moment concern us; the position is that *on a theory of "formal" necessity*, we cannot but admit that the way in which the proposition is known is always known along with it; or, more generally, that the categories do qualify, or form an essential part of, the proposition asserted. But a question that arises now is whether the modal content of a proposition can be taken to be the way in which it is related to the faculty of cognition; whether "being experientially known", or "rationally known" can be part of the content of any proposition. Of course, even if it is admitted that this can be so, all difficulties do not vanish, because we still may ask whether a proposition is formally known as formally known, or empirically known as formally known.

In the latter case, the necessity of the proposition would be contingent; *i.e.,* the proposition would not be necessary at all, since we could conceive the proposition as contingent, and, therefore, conceive a significant contradictory of the proposition. We must say, then, that the proposition's being rationally known is rationally known, but this leads merely to an infinite regress. We might, perhaps, arrest the regress in this case by contending that its being rationally known, and its being known as rationally known are identical—*i.e.,* that the rational knowing knows itself. But this solution is not available in the case of contingency. Here, the proposition is known as empirically known, but it cannot be empirically known as empirically known, since that would create the possibility of our being mistaken as to its being empirically known, *i.e.,* the proposition might be rationally known, in other words, it might have no possible contradictory and, therefore, not be contingent. Yet, we can scarcely speak of a proposition's being empirically known, and its being rationally known as empirically known as identical. It would appear, then, that we cannot treat the way in which the proposition is known as being an essential part of its content; and if "formal" necessity must be part of the content of the proposition, then the categories of modality cannot indicate "the relation of the object to the faculty of cognition".

We must pass, then, to consider in what the modal property of propositions might consist. Johnson speaks constantly of the "modal adjectives", and takes modality, then, as predicate qualifying the whole proposition. However, the criticism already passed upon Johnson shows, I think, that this view cannot be maintained along with a theory of "formal" necessity. In the case of truth, *e.g.,* which Johnson classes provisionally with the modal predicates, if we say that "true" adds anything to the content of a proposition, we should never be in a position to assert finally that a proposition is true. For if we maintain that "A is B is true", then, on this view, we should have to assert further that the proposition "A is B is true" is itself true. Here, it is obvious, we have no additional proposition. Again, the assertion that "A is B is necessary" would not give us the necessity of the proposition "A is B", since, unless the compound proposition is itself a necessary proposition, then, as we saw, a contradictory to it would be possible, and thus there would be a significant contradictory of "A is B". Here, also, we get an infinite regress, which can be avoided only by recognising that the assertion "A is B is necessary" is not one that attributes an additional quality to "A is B". Or, in other words, it is not the case that we have the modally characterless proposition "A is B" and assert necessity of it, as we assert a predicate of a subject, but that the modality is essentially involved in the proposition so that it cannot be denied except by the assertion of a different *"primary"* proposition. It will not, we may say, be a question of whether "A is B" has, or has not, the character of necessity, but rather, whether "A is necessarily B" is, or is not, true *as a whole.*

We can agree, of course, that Johnson gives the whole doctrine an appearance of plausibility by treating the question of modality as one of propositions about propositions. But, as we saw, the assertion that a proposition is necessary is not what is meant by a "formally" necessary proposition. If a proposition is necessary, it is not that it has a character that may or may not be affirmed of it, but it must be that the proposition has a character that cannot be denied of it. This is, perhaps, merely to say that a necessary proposition is one that cannot be denied—in this resides its necessity. Now, this denial of the contention that necessity is a character that can be ascribed to propositions leads to important conclusions. In the first place, it will not be legitimate to affirm that any proposition known by experience is necessary, unless necessity is "inherent" in it; unless, *i.e.,* it is impossible to affirm its contradictory. If we say, for example, that the angles of a triangle equal two right angles, it will not be sufficient to add that that proposition is as a matter of fact, necessary, since that second assertion might be denied. And, to repeat once more, the assertion of the contingency of the proposition would imply the possibility of a contradictory to it, and while, therefore, it might be necessary in the epistemic sense, it could not be formally necessary.

Now, it is doubtful whether anyone has asserted that there is any matter of fact that could not be significantly denied. Johnson, of course, speaks of laws of nature as *nomically* necessary, *i.e.,* necessary in the way that laws of nature are necessary; but he does not imply that these are formally necessary. But even if it were contended that necessary matters of fact are "formally" necessary, this would not avail. For, as we have seen, to say that certain propositions are transparent to, or certified by, reason, does not tell us how to recognise those that are, and the reason, we may assume, is not arbitrary in its choice. In fact, in the doctrine of "formal" necessity, we have the same attempt to have things both ways as we have in the doctrine of the analytic proposition. If a proposition simply were analytic, if the predicate merely were contained in the subject, that would be a pure matter of fact, and there would be nothing to show that the predicate must always be contained in the subject. Hence, it is clear, it would have to be argued that the analytical nature of the proposition is constituted by its necessity, and not its necessity by its being analytic. The same may be said of any similar attempt to account for necessity. Thus, as regards Meinong's theory[1] of the inherence of factuality in an objective, in the case of necessity, and its adherence to an objective, in the case of brute fact, there is nothing in the relation of inherence that implies necessity; factuality may or may not inhere, or it may or may not adhere, *i.e.*, inherence, unless it is *necessary* inherence (in which case we get another regress), is as much "brute fact" as is adherence.

It would appear, then, in the first place, that unless "laws of nature" are "formally" necessary (and not even then) they are not necessary at all. Or, more precisely, they are not situations that cannot be conceived otherwise. We may state the conclusion in this way, that no sort of necessity considered simply as a predicate actually asserted of propositions could give absolute necessity. If A's being B requires a law of nature to necessitate it, then that law's being necessary will require something to necessitate *it,* unless the proposition, "This law is necessary" is itself a *"formally"* necessary proposition—*i.e.,* one that cannot formally be denied. Or, it might be contended, the proposition "A is B" is simply a fact, neither necessitated by "law" nor self-necessitated, and "A is B is necessary" is a fact of the same absolute sort. But all that this means is that a certain relation holds between A and B, of no different kind from that which holds when A is B is not necessary; in either case, the relation will be a contingent matter of fact, and not a "necessary" relation, in the sense of formally necessary. And, what relation does hold between the two, we can discover only by experience. Thus, if it is said that "all the Sundays in July were wet", we know that we can verify this proposition by investigating each of the Sundays in turn, since we know that the relation prevailing between the two terms is not of the kind that holds when we assert that "all men are mortal". We can call the second relation one of necessity, if we like; but only so long as this is not taken to imply that the relation is "absolutely" or "formally" necessary—*i.e.,* that the relation is self-necessitating in such a way that no other relation could hold, under any conditions.

We may, then, conclude that the conception of formal necessity is to be rejected; we can find no proposition that necessitates itself or its own assertion. Thus, we can hold that while Johnson's view of the modal categories as predicates contradicts the theory of "formal" necessity, nevertheless, that is the only view that can be maintained. From this it will follow that necessity is a character of certain things, and (putting aside the doctrine of different degrees of existence, which is required only by a theory of "formal" necessity) we should say that necessity is a certain sort of relation between things. But here we may distinguish two relations, each of which may be taken to be necessity. We have, first of all, the relation referred to at the beginning; a thing's being necessary in the sense that it is necessitated. As we saw, epistemic necessity—a proposition's being necessarily affirmed—can be taken to be of this sort. Secondly, there is the relation that holds when we say that "All men are mortal".

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[1] My knowledge of Meinong, on this question, is derived solely from J. N. Findlay's book *Meinong's Theory of Objects.*

Now, the recognition that nothing is self-necessitated implies that everything is contingent; *i.e.*, that everything is necessitated by something different from it. Hence, the necessary relation, or the relation of universality, as we may call it, will be itself necessitated, or contingent. And, again, when the relation between two things is not the relation of universality, nevertheless, we must contend, it is necessitated. The question arises, then, as to what account of possibility can be given in these terms. We may remark, at the outset, that when Kant distinguishes between intellectual and empirical contingency, the distinction he is making will correspond to one between that which is not self-necessitated, and that which is actually necessitated; and, if I am correct, no distinction remains to be made. We are left only with the empirically contingent. It will follow, in the same way, that we must reject the possibility which is defined as that which does not contradict the intellectually necessary, and we are left again only with the empirically possible. I should contend, however, that this is a contradiction in terms; that, in fact, no doctrine of possibility can be held, unless based upon the conception of "formal" necessity. Corresponding to the three sorts of necessity Johnson distinguishes there are three kinds of possibility. Epistemic possibility, I have already referred to. "Formal" possibility (the non-contradiction of a "formally" necessary truth), I should say, means nothing. The nomically possible is that which does not contradict a natural law. We should have to say, then, that the possible is that which does not contradict facts between which prevails the relation of necessity, or universality. But it is difficult to see what this can mean. Obviously we can say that "some men are mortal" is possible, because it does not contradict the proposition "all men are mortal", but that means merely that whatever is, is possible. What we have to consider is whether the possible is wider than the scope of the actual. Now, we may argue that it is possible for some books to be printed in red ink, though none ever have been, since there is no necessary relation of exclusion (*i.e.*, universality) between "books" and "being printed in red ink". This seems to be the only remaining account of possibility that could be given, and it can be shown, I think, that this must be rejected also.

We should have to argue, that is, that it is not necessary for books to be printed in black ink. But this statement is, at most, true in only one of the two meanings of necessary we have distinguished. It may be argued that the relation between the two terms is not the one that holds between the terms of mathematical propositions. But even admitting this does not establish the possibility of the thing. Although, let us say, the mathematical proposition expresses a relation of necessity and the proposition that "all books are printed in black ink" does not, the relations are equally contingent—*i.e.*, the relation expressed by the proposition in mathematics does not necessitate itself, but is contingent upon the existence of certain other conditions. A relation of necessity, as we argued, is a peculiar relation that exists in exactly the same contingent way as any other. There is, therefore, no self-contradiction involved in supposing that a certain relation is not the relation of necessity, or, further, that the proposition which expresses that relation is not true.

We can conclude, provisionally, that it is possible for books to be printed in red ink, for the reason that nothing is impossible. But the conclusion that nothing is impossible would, I think, be defended by no one. Assuming, then, that certain things are impossible, it is necessary to show why this is so, and what light it throws upon the question of possibility. It is not because a proposition contradicts a "formally" necessary one that it is impossible. Nor is it *merely* because it contradicts a proposition which asserts the relation of necessity to hold between two terms; there is, as we saw, no self-necessitating reason whereby that relation should be one of necessity, or why another relation should not, in fact, prevail. But, while there are no self-necessitating reasons, there are reasons. The existence of the relation of necessity is necessary, but necessary in the same way as everything else. Hence, when it is said that a proposition "A is B" is impossible, all that can be meant is that there is nothing which necessitates the situation "A is B", or that, under the circumstances, "no

A are B". I should conclude, then, that rejecting the conception of the self-necessitating, and consequently, that of the self-necessitation of propositions affirming a relation of necessity or universality between terms, impossibility can mean no more than the lack of "a sufficient reason".

Therefore, we can conclude, whatever does not exist is impossible, and the only thing possible is what actually exists. The treatment of certain non-existent things as possible is an attempt to confuse the various meanings of necessity. The possible is not real because there is no "sufficient reason", but it is possible, because it does not contradict the necessary. But if the necessary is the necessitated, the caused or conditioned, then the merely possible does contradict it. On the other hand, if by the necessary is meant the relation of necessity, then many of the things actually existing would be impossible. The proposition that "some men are unhealthy", contradicts the proposition "all men are necessarily healthy". But there is no necessity for the existence of such a necessity; *i.e.*, unless conditions exist sufficient to produce it. Thus, we return again to the point already mentioned too frequently, *viz.*, that we can admit a relation of necessity without defending the doctrine of necessary truth, or necessary being. Such a position implies that the question of modality is much simplified. We have, then, necessity (or universality) and its opposite; but no category of possibility, and no category of contingency as applying only to certain things. The older rationalists like Descartes and Leibniz were correct at least in recognising that the traditional theory of the modes can depend only upon a self-necessitating being, and self-necessitating truths.

NOTES ON ÆSTHETICS.

By R. P. Anschutz.

"These fragments I have shored against my ruins."

(1) It is popularly supposed that philosophy begins at the beginning. But that is an error: it begins in the middle. An inquiry—any inquiry—becomes philosophical when it turns back to clear up ambiguities in the apparently simple ideas with which it started. But it is unnecessary to turn back until some progress has already been made, and it would be pointless to turn back except to make further progress.

Philosophy is nothing but a stage—a continually recurring stage—in the development of science. And the only defence that can be offered for it is that it is a necessary stage.

It is the accurate description of particular things and the particular relations between particular things that constitutes knowledge. There is nothing else of which there can be knowledge and that knowledge is science. But it seems impossible, as a matter of fact, to pursue the detailed investigation of particulars in a straightforward way. After each bout of fruitful detailed work men have to take stock of the terms at their disposal before they start on another bout. And it is these reflective interludes that constitute philosophy.

Every science, then, becomes philosophical at its growing point, because it is a condition of further work in any science that men should ask themselves again what they have been dealing with all along. But it is no use trying to be philosophical without some interest in and some knowledge of the particular facts that have provoked the re-examination of the general terms. It is only the reflections which have grown out of and are pursued to further the detailed work of any generation that contribute anything valuable to its philosophy. And that is why the public chooses to regard Messrs. Russell, Whitehead, Eddington and Jeans as the representatives of English philosophy in this generation and not those who sit in its high places.

(2) Æsthetics is to criticism what the philosophy of mathematics is to mathematics or the philosophy of physics to physics. It is an attempt to overhaul a system of

terminology in order to secure a more accurate description of a certain class of facts.

There is an immense difference, of course, in the status of criticism and mathematics, and it is only reasonable to expect a corresponding difference in the refinements of æsthetics and the philosophy of mathematics. In the one case it is only the expert who realises the necessity for any overhauling of terminology. The plain man who uses arithmetic in his office and geometry on his tennis court is quite satisfied with the existing system. In the other case it is obvious to everyone that most attempts to characterise such things as novels and paintings and films are miserable failures. We simply don't know what to expect when we read a critic for the first time. We can only hope that his taste is consistent and try to discover in course of time to what extent and in what directions it differs from our own.

But although it is scarcely possible to exaggerate the deficiencies of criticism, there is no other possible point of departure for an æsthetics which is aware of its modest place in the scheme of things. It is these very deficiencies of crticism which define the current problems of æsthetics, and it is only through their removal that any theory of æsthetics can be verified. A theory of æsthetics which is not illustrated, or at least not capable of illustration, at every point by detailed criticism is not a theory of æsthetics at all.

And the worse the state of criticism, the closer æsthetics must cleave to it. It is no doubt useful to know what Thomas Aquinas said about beauty (and how little he did say), but in the present state of the inquiry it is far more useful to know exactly how Roger Fry thinks he can support an assertion that one picture is beautiful and another is not.

(3) Æsthetics, then, should do for criticism what Mr. Culbertson has done for Bridge. It should analyse the subconscious processes of the expert so that they may be learned by the less gifted. But that, of course, assumes that there are experts in criticism as in Bridge. And further, either that the æsthetician is one of them or else that he is capable of recognising them when he meets them. But is that assumption justified? Mr. Culbertson's claim to be a Bridge expert is substantiated by his consistent success in winning matches. But how is anyone to be recognised as an expert critic? What observable difference does it make if anyone is an expert critic?

The obvious answer is that as in Bridge so in criticism, the test of knowing is in doing. The expert critic is the expert artist. But that answer unfortunately has been falsified whenever it has been tested. Socrates found that the poets he questioned knew even less about their poems than other people. And Mr. Casson's more recent inquiries among plastic and pictorial artists for the B.B.C. lead to the same conclusion. The artist can talk informatively about the processes of production but not about the product itself, or at least no more informatively than other people who are not artists.

"There are two reasons", says Mr. Eliot, "why the writer of poetry must not be thought to have any great advantage. One is that a discussion of poetry such as this takes us far outside the limits within which a poet may speak with authority; the other is that the poet does many things upon instinct, for which he can give no better account than anybody else. A poet can try, of course, to give an honest report of the way in which he himself writes: the result may, if he is a good observer, be illuminating. And in one sense, but a very limited one, he knows better what his poems 'mean' than can anyone else; he may know the history of their composition, the material which has gone in and come out in an unrecognisable form, and he knows what he was trying to do and what he was meaning to mean. But what a poem means is as much what it means to others as what it means to the author; and indeed, in the course of time a poet may become merely a reader in respect to his own works, forgetting his original meaning—or without forgetting, merely changing."

(4) But if the artist doesn't know, who does know? Who can know? Aren't we driven to the conclusion that criticism is not a matter of knowledge at all? And that therefore it is absurd for anybody to claim to be a better critic than anybody else?

There are a dozen tags to attest the popularity of this contention—De gustibus non disputandum, Quot homines tot sententiæ, Chacun à son gout. But it is another question whether anybody has ever really believed it. Every tag which asserts it can be matched by a dozen books which deny it. And nobody I imagine who understands what he is saying can be *merely* amused by the experimental method proposed by Mr. J. B. Watson for solving all questions of taste.

"If I had to pass a critical judgment upon a work of art", he says, "a picture, for example, I should do it experimentally. I should arrange to let crowds of people from all walks of life wander one at a time into a well lighted room. I should have visual stimuli about such as magazines, knick-knacks of one kind or another, two or three pictures on the wall, including the one I wanted to have judged. If an individual under observation spent time at this picture, if he showed some emotional reaction such as grief, joy, rage, then I should put him down as reacting positively to it. At the end of the day I should be able to say: 'The so-called critics will say your picture is terrible, the children will not look at it, the women are shocked by it, but the travelling salesmen chuckle with glee over it. It will be a failure if you exhibit it: I should advise you to send it to some sales manager and let him hang it over his desk'."

The general feeling, I imagine, will be that however useful Mr. Watson's experiment might be in brightening the lives of travelling salesmen, it would certainly not enable him to pass a critical judgment upon a picture, which is what he set out to do. And something similar will be felt, I should think, about the experiments which Mr. I. A. Richards has actually carried out with poems and described in *Practical Criticism.* The comments he records are interesting enough as instances of the ways in which poems may be misread. But that scarcely justifies the title of his book, nor the claims which have been made for his method. One might just as well publish the answers of an infant class to a set of simple arithmetical problems under the title "Practical Arithmetic" and claim that that constituted a revolutionary advance in the science.

(5) The belief that criticism is a matter of knowledge and that therefore there can be expert critics has been traditionally expressed in the use of the word 'beauty'. And Kant's elucidation of the distinction between the beautiful and the agreeable remains the classical statement on this point even although he didn't believe in it himself.

"As regards the *agreeable*", he says, "everyone concedes that his judgment, which he bases on private feeling, and in which he declares that an object pleases him, is restricted merely to himself personally. Thus he does not take it amiss if, when he says that Canary wine is agreeable, another corrects the expression and reminds him that he ought to say: It is agreeable *to me.* The beautiful stands on quite a different footing. It would be ridiculous if anyone were to think of justifying himself by saying: This object is beautiful *for me.* For if it merely pleases *him,* he must not call it *beautiful.* When he puts a thing on a pedestal and calls it beautiful, he demands the same delight from others. He judges not merely for himself, but for all men, and then speaks of beauty as if it were a property of things."

(6) A hundred years ago it would have been possible to define a critical judgment as one that asserted or denied that a thing was beautiful. But Tolstoy and Hegel have changed

all that and now there is no term that is more apologetically used in criticism or more suspect in æsthetics. The only philosophers who still retain a belief in beauty (*i.e.,* a belief in the significance of any proposition of the form: X is a beautiful thing) seem to be the neo-scholastics. And that, of course, only makes it easier for the others to brand it as a superstition. "Even today", says Mr. Richards, more in sorrow than in anger, "even today, such is the insidious power of grammatical forms, the belief that there is such a quality or attribute, namely Beauty, which attaches to things which we rightly call beautiful is probably inevitable for all reflective persons at a certain stage of their mental development."

And the dogma of the dispensability of beauty is not only asserted by the most influential philosophers, it is also accepted by the most influential critics, even by those who might have been suspected of a different opinion. Roger Fry may attack Mr. Richards, but he still defers to Tolstoy, and Mr. Eliot defers to Mr. Richards.

And it seems further to have secured a considerable degree of popular support. I am ignorant of the extent to which the adjective "beautiful" was popularly used in any serious sense before the days of Hegel and Tolstoy—apart from Tolstoy's own evidence about its increasing use in nineteenth century Russia. But there is no doubt of the reluctance with which it is used now. Any other adjective seems to be preferred—"splendid", "good", "fine", "first rate", even in some circles "amusing" and "thrilling". But the favourite expression is certainly "work of art"—"That *is* a work of art", or "That is a *real* work of art". And on the other hand, where "beautiful" is used, it is only rarely that it is used seriously. The assertion that a book is beautiful, for instance, is generally made by the same sort of woman who asserts that a book is sweet, and with much the same meaning.

It may be objected that there is a much simpler explanation of the popular avoidance of "beauty"—that it indicates merely that people are not prepared to take their critical judgments seriously, and hence prefer to avoid the arguments that seem to be invited by the use of "beauty". But it is fairly plain that arguments about beauty are avoided chiefly because they are regarded as a waste of time. And that, of course, is precisely the contention of those who propose to eliminate "beauty" from the vocabulary of criticism. It is impossible, they hold, to reach any agreement about beauty and so the term, being purely otiose, is better dispensed with.

The thing to do is to discuss art—the creative imagination of the artist, the recreative imagination of the connoisseur, the function of the artist in society.

(7) Now the disagreement about beauty is to be cherished, I suggest, in the first place because it is a disagreement, and in the second place because it is a fundamental disagreement. It provides a rare opportunity for getting the different schools together to talk to the same question. And that question is the first question of æsthetics: What are we talking about in criticism?

It is no doubt simplifying the issue pretty considerably to say that the only important question that has to be decided in æsthetics is whether propositions of the form "X is a beautiful thing" are significant (*i.e.,* capable of truth or falsity). But it is pretty certain by now that philosophy can only be useful to criticism (or anything else) when questions like this are under consideration. It has to try to find the gist or pith of a tangle of conflicting considerations in the use, misuse or disuse of a few elementary ideas, concepts or categories (call them what you like) like goodness, truth and beauty. That is what it is for, and that is all that it can do.

It is only rarely, of course, that the results have been valuable. You can count the philosophers who have really cleared things up, on two hands: Plato and Aristotle, Descartes and Leibniz, Locke and Hume, Kant—who else is there?

Philosophy is a dangerous occupation demanding a superhuman combination of qualities if it is to be successfully carried through and involving as the penalty of failure about the most awful fate that can happen to any man——the savage and solitary gnawing of bones that have been buried for years and years and years. That is why so many professional philosophers find their life's work in academic administration, rather than in philosophy. And a good thing, too.

But if anyone wants to pursue an occupation he has to take his chance with the occupational disease.

(8) The trouble with the contemporary situation in æsthetics is not that the authorities disagree. If they disagreed seriously enough they would have to define the point of their disagreement and then there would be some chance of forcing an agreement. At least they would have to agree about the point at issue.

The trouble is rather that the authorities ignore each other and the reader doesn't know what they are talking about for the simple reason that they are all talking about different things.

Consider, for example, the state of literary criticism and the æsthetic theories (the great majority, by the way) which are primarily based upon the consideration of literature. There are two neatly definable philosophical schools with fully authenticated pedigrees:

(i) *The Idealist School*—Kant (who deserves a better fate), Hegel, Croce and a few academic interpreters.

(ii) *The Moralistic School*—Bentham (as interpreted by Mr. Richards), Tolstoy, Richards and dozens of very earnest disciples.

And then there are:

(iii) *The High Brows*—notably Eliot, Pound and Stephen Daedalus. They are more of a literary group than a philosophical school, but they take their æsthetics seriously, and they have other things in common. And their claim to a respectful hearing is certainly not based merely on the fact that they include the most considerable writers in English.

Now the reader who is trying "to survey the contemporary situation" has to read representatives of all these groups. And his initial difficulty is the general absence of cross references apart from a few vaguely directed sneers. Croce, for instance, seems to be completely unaware that people with the opinions of Richards and Pound exist or could exist. He doesn't seem to realise that they offer the contemporary alternatives to his own doctrine. And similarly with Richards and Pound, though Richards is rather better in this respect (the only respect, I think) than Pound.

Now why is this? You don't find physicists or physiologists or even psychologists behaving like this. When they are discussing a question at any length they are careful to define the relation of their solution to the solutions of their illustrious predecessors and contemporaries. They may not talk very much of the continuity of the scientific tradition, but they act as if they believe in it. But these others who are always talking of the continuity of literary tradition never do anything to substantiate it in their own work. Why is this?

The answer seems to be that though all these groups consider that they are talking about the same thing—criticism or æsthetics—they are nevertheless talking about entirely different things. The Idealists are talking about Idealism, the Moralists are talking about Morals. It is only the High Brows who are really trying to talk about literature. The others are using literature as an excuse for talking about something else—the non-existence of physical things or "the desperate plight of this generation".

(9) The first definition of the beautiful given by Thomas Aquinas (and concurred in by Kant and many others) is *quod visum placet*—what pleases on sight, *i.e.,* immediately. And while this is certainly a bad definition, it does, I think, provide a convenient point to begin with when it is properly understood.

It is a bad definition simply because it is not true: a thing may be beautiful without pleasing anybody. But on the other hand, it is true, I think, that if a thing pleases anybody on sight, then it is beautiful. And so although *quod visum placet* does not describe all beautiful things, yet it does identify the things which have been recognised as beautiful. And that is sufficient to get the investigation going.

But how is it to be understood? There are two terms to interpret—*visum* and *placet.*

(a) *Visum* (which has been translated *immediately*) does not mean that a thing in being recognised as beautiful has to please *instantaneously* but that it has to please *disinterestedly.* It does not rule out a time interval in which the thing does its work on us, but it does rule out a time interval in which we are considering its bearing on our own ends. "Immediately", then, has to be understood in the primary sense in which it is opposed to "mediation", not in the secondary sense in which it is opposed to "delay". It means "directly", that is, it rules out the sort of enjoyment that is obtained by Mr. Watson's travelling salesmen.

(*b*) *Placet* has been translated as "pleases", but it is really more important to retranslate "pleases" as *placet.* If the formula *quod visum placet* is to be of much use, we must be able to say that a thing pleases (or does not please) a person even in the absence of his own report. So much may certainly be conceded to Mr. Watson. We cannot accept everything a subject says on trust: we must be able to verify it. And so we must be able to decide whether it pleases him merely by watching him (over a sufficiently long period) to see whether he goes on looking at this thing, or reading it or listening to it.

That is the only way in which we can meet the common objection—Tolstoy provides a typical statement—that our formula, being subjective, is therefore not objective. We can now answer that, although we are attempting to identify beautiful things through the human reactions they provoke (subjectively?), yet those reactions are as objective (*i.e.,* as commonly observable) as anything else in the world.

And there is not, I think, any reason to fear that this behaviouristic interpretation of "pleases" will rob the term of much (or any) of its meaning. The phrase "it pleases me" is indeed often expanded into "it gives me pleasure", as if "pleasure" were something which could be talked about apart from action. But nobody, I think, who has tried to talk about it in this way, or who has seen the trials of others, will give much credit to that assumption.

(10) Things, then, are called beautiful, or have been called beautiful, when people look at them or listen to them or read them—and continue to look and listen and read—apart from any interest. And an interest, it may be added, is as observable a fact as a looking.

It follows from this that there is a good deal to be said for those critics who lean hard upon the judgment of posterity. Lapse of time considerably reduces the number of points at which a thing affects our interests. And so the selection that people make among old things is more likely to correspond with their value than the selection they make among new things. They are less likely to be deceived by the operation of an unacknowledged motive.

It is not, of course, quite so easy as it sounds. A man is still liable to be influenced by the fact that Poussin was a Frenchman, Rubens a Catholic and Van Gogh mad, even although they are dead. It is difficult not to underestimate the last fashion (in literature, or clothes, or anything else), and perhaps even more difficult not to overestimate the last fashion but one. This seems to be true whatever the lengths of the fashion periods that are being considered—whether it is five-year cycles in women's clothes, or hundred-year cycles in poetry. Antiquarian interests, again, are often disturbing. And there is a further difficulty peculiar to literature which is even harder to overcome—that of appreciating the "weight" of a writer's words as they were appreciated by his contemporaries.

But, by and large, there can be little doubt, I should think, that time does tell and that the safest of all critical methods is the wait-and-see method. The trouble is, of course, that we can't wait very long—fifty years or so—and if we all waited, we should have nothing to wait for.

What we have to try and do in æsthetics, then, is to short circuit the wait-and-see method of criticism so that we can predict now what people will still be reading in a hundred years' time in spite of the change of fashion. And we can only do that by investigating the things that people continue to read and re-read.

(11) "Literature", says Mr. Pound, "is news that stays news." But the first question that we have to decide is whether there is any news that stays news. And, strange as it may seem, it is pretty plain that Mr. Pound doesn't believe that there is.

A thriller, Mr. Pound points out, can only be read once, however good it is. And it doesn't matter, we may add, whether it is fiction or fact—like Cummings' *Moscow Trial* or Dearden's *Fire Raisers.* On the other hand, Joyce's *Ulysses* is continually being re-read. Is this difference to be credited to any difference in *news* value? It is perfectly obvious that it is not. So far as news value is concerned, I am not at all sure that the *Moscow Trial* and the *Fire Raisers* shouldn't both be rated higher than *Ulysses.*

Or again, take Shakespeare—just take Shakespeare. There has never been an effective answer to Tolstoy's attack on his work in general, and on *Lear* in particular. And there never will be. Tolstoy is perfectly right in maintaining that the action is blurred, arbitrary and often downright silly. The news value of a good many of Shakespeare's plays is as near zero as can be. But there is no doubt, I suppose, that Shakespeare is still read.

But if we don't re-read books for their news value, why do we re-read them? For the sake, I believe, of the surface value of their crust of words; for the way in which their words are charged with a meaning (though meaning isn't the proper term either) which isn't news value. And that, of course, is what Mr. Pound believes, too.

There was never a more foolish prayer than Katherine Mansfield's "O Lord, make me crystal clear". It is the perfect reporter who is crystal clear, and however avidly he may be read in the first place, he stays unread ever after. The writer who wants to be re-read has to write something which will exist in its own right and not merely as a vehicle for reporting. His stuff has got to be opaque, and it can't be opaque if he is crystal clear.

(12) The suggestion that we do not re-read books for their news value is confirmed by the way in which we do read books for their news value. We are greedy, we gulp, we won't go to bed, although we know we ought to. And we behave like this because, like children, we are only concerned with what is going to happen next. We don't do this with a book which we recognise as a "good book", *i.e.,* with a book which we think we may re-read.

Arnold Bennett was certainly not the fool that a lot of people have tried to make out. But he did have his blind spots, and they are particularly noticeable in his occasional attempts to generalise about literature. Here, for instance: "Incomparably the greatest obstacle to pleasing the middle class lies in the positive fact that it prefers not to be pleased. It undoubtedly objects to the very sensations which an artist aims to give. If I have heard once I have heard fifty times resentful remarks similar to: 'I'm not going to read any more bosh by *him*! Why, I simply couldn't put the thing down!' It is profoundly hostile to art, and the empire of art. It will not willingly yield. Its attitude to the magic spell is its attitude to the dentist's gas bag. This is the most singular trait I have discovered in the backbone."

In this, I think, Arnold Bennett shows himself considerably less sensitive to "art" than the "class" he is attacking. Otherwise he would have known that this resentment at a book one cannot put down is a resentment not at the "magic spell of art" but at one's own weakness for scandal. I should imagine that it is only the second-rate artist who aims at keeping his readers continually on their toes to see what happens next. But however that may be, I am quite sure that it is only when readers consider that they are dealing with a second-rate book that they actually get on their toes. We are not hurried forward by first-rate work; we lean back and savour it.

(13) But what exactly is it that we savour?

Words are not merely transparent vehicles of dictionary meanings. They have properties of their own—they have sounds and they have associations. It is suggested that writing is re-read for the sake of these properties.

"Queens have died young and fair."
"La fille de Minos et de Pasiphaé."

People can go on repeating these lines for the sake of the sound and nothing but the sound. That is shown by the fact that the second will still have something of the appeal of the first, even when the words convey nothing.

"What is surprising about the poetry of Dante", says Mr. Eliot, "is that it is, in a sense, extremely easy to read. It is a test (a positive test, I do not assert that it is always valid negatively) that genuine poetry can communicate before it is understood. The impression can be verified on fuller knowledge. I have found with Dante and with several other poets in languages in which I was unskilled, that about such impressions there was nothing fanciful. They were not due, that is, to *mis*understanding the passage, or to reading into it something not there, or to accidental sentimental evocations out of my own past. The impression was new and of, I believe, the objective poetic emotion."

But if something of a poem can get across to a person who is unfamiliar with a language, there is also something else that can only get across when a person is *thoroughly* familiar with the language. Knowing the pronunciation of the words and very little else, it is possible to obtain a more adequate idea of a poem than of a text book, but the text book will certainly be properly understood long before the poem. For the text book one needs only the dictionary meanings of the words, and the more recondite the subject, the more narrowly its terms are defined. But for the poem one has to feel the history of the language behind its words and the history of the people behind the history of the language.

(14) "Numerals and words referring to human inventions", says Mr. Pound, "have hard, cut off meanings. That is, meanings which are more obtrusive than a word's 'associations'. Bicycle now has a cut off meaning. But tandem or 'bicycle built for two' will probably throw the image of a past decade upon the reader's mental screen."

You have to know a language pretty well before you can appreciate the difference in the weight of those two words—bicycle and tandem. But you certainly won't re-read much of its poetry until you do. I can appreciate Bloch's preoccupation with "La fille de Minos et de Pasiphaé". But its appeal is very thin compared with "Queens have died young and fair". Four of these six words—queens, died, young, fair—are almost rotten with associations. But Minos is just bicycle to me.

"Years ago", says Mr. Pound, "a musician said to me, 'But isn't there a place where you can get it all (meaning all of poetry) as in Bach?' There isn't."

The reason why there isn't is, of course, that words get saturated with associations in different ways in different languages. And that is the justification for Mr. Pound's continued insistence that you can't appreciate literature without knowing the literature of several languages. What do they know of England who only England know?

But the trouble with Mr. Pound is that he does not seem to realise the difficulty of the programme he is proposing. He says, for example, that there is so little of Sappho that one might just as well read her as not read her. But it would be impossible, I should have thought, not knowing the lady, to make very much of her when she was all one knew. It would be necessary, I should have thought, to know Greek to know Sappho—and to know Greek inside out.

(15) According to Mr. Pound words are charged or energised in three ways over and above their plain dictionary meaning—by their sound, by the visual images they call up, and by the associations which they derive from their habitual context. And so there are "three kinds of poetry" (his quotation marks)—Melopœia, Phanopœia and Logopœia.

I think that Mr. Pound exaggerates the importance of visual imagery. And I doubt very much whether it is possible to distinguish so sharply between the poetry which calls up visual images and the poetry in which the associations make their presence felt in other ways. The elements in a poem that appeal visually to one person may easily appeal auditorily or kinæsthetically to another. And the same person may alternate between different kinds of imagery: he may start using visual images, slough them after a time and then re-discover them when he is cross examined.

And I doubt further whether it is possible to find any writing which is continually re-read merely for the sound, or merely for the richness of its associations. I should say that both factors are always present together. I do not know whether Mr. Pound would deny that. He certainly puts the phrase "kinds of poetry" between inverted commas. But he does use the phrase, and it is better when one agrees with so much of what a man says to be quite clear what one is agreeing with.

(16) But if it is a common weakness of humanity to take things at their face value, it is a still commoner weakness not to take them at their face value. People like to think deeply, because it absolves them from observing widely—in some cases from observing at all. If you have grasped the root of the matter, it is a waste of time to number the twigs and leaves. And, what is still more consoling, you have every justification for looking up with contempt at the industry of those who are busy among the branches.

Now this way of approaching a subject, which is known in philosophical circles as Rationalism, is not very often met with now—outside philosophical circles. In the old days, of course, it was used to solve every problem. If you wanted to know what happened when sulphuric acid was poured on iron, the last thing you thought of was pouring sulphuric acid on iron and seeing what happened. It was far more conclusive (and, incidentally, far easier) to go to the root of the matter and, considering the essences of sulphuric acid and iron, deduce what must happen.

But apart from mathematics (perhaps) and politics and theology (of course), there isn't very much Rationalism about now. People are more and more inclined to accept a superficial view of things. And some people are even prepared to go out of their way to look at things instead of sitting back and thinking them out.

Except in æsthetics. The destructive Mr. Pound may say: "The proper method of studying poetry and good letters is the method of contemporary biologists, that is careful first hand examination of the matter and continual comparison of one 'slide' or specimen with another. . . . By this method modern science has arisen, not on the narrow edge of mediæval logic suspended in a vacuum."

But in æsthetics people are still bemused by the fancies of Georg Wilhelm Friedrich Hegel. They still believe that the particular lines, curves, surfaces, colours and so on of a work of art are of little importance because "its function is to unveil an inward or ideal vitality, emotion, soul, a content and mind". And that is why most talk about "art" is still an excuse for edification.

"An art gallery", says His Excellency Lord Bledisloe, late Governor-General of New Zealand, "which does not draw within its walls the general intelligent public of all classes of the community through the consciousness that it echoes its highest natural aspirations and supplies the ethical and spiritual pabulum for which its soul ardently craves, fails to justify public support."

UNIVERSITY REFORM.

By John Anderson.

Reform of the University will be differently understood according as one starts from a commercial or from an academic point of view, and further differences will be occasioned by the acceptance of a solidarist or a pluralist view of society. It would appear, however, that even those who hold that the question is to be settled in terms of the University's "ultimate value to the community", will require in the first instance to take account of the actual character of its work, and of its position as a special kind of social institution; they will have to consider the nature and conditions of *academic* activity or, at the very least, of "research". And even the most bigoted solidarist will hardly deny that institutions for "the higher learning" have a history of their own and that, however they have interacted with other institutions, their development has been conditioned by an independent interest in investigation and has not been brought about by successive decisions of "society".

It is to be understood, of course, that the interest in investigation is not confined to Universities, and, at the same time, that it does not function unhindered within them. But this merely means that, for those who are devoted to inquiry, University reform will consist in the strengthening of the forces of inquiry within the University and their closer alliance with similar forces outside, and that projected "reforms" in any other direction are in reality reactionary. The point to be emphasised is that the adoption of either the solidarist or the commercialist position (and the two soon run together) can neither account for the origin of the spirit of investigation nor guarantee its continuance; subordinated to "welfare" or to profit, science perishes. Certainly, the "results" of investigation can be of great commercial value; certainly, the problems of industry can give an impetus to inquiry. But, unless the independent scientific spirit exists, such interrelations cannot continue—and, incidentally, in such a case industry itself will decline.

The first condition of the maintenance and strengthening of the academic spirit is publicity. This means not merely that the public should have information on University activities, but that the academic or cultural point of view should be propagated among the public. It is the business of academic investigators to speak out on behalf of their way of living and not, as is so often done, to apologise for it on the ground that after all it is of some assistance to non-academic enterprises. In this way they would not only arouse the cultural attitude in places where it is not at present active but would establish relations of solidarity with extra-academic investigators. They would aim, of course, not merely at enlisting formal support for culture but at carrying culture beyond the University. As far as present efforts in this direction are concerned, they have no more than an edifying, sermonistic effect; what is wanted is the encouragement of independent investigators, the organising of groups of serious students, and (what all this involves) an active intervention in public affairs.

Even in a matter so closely, indeed essentially, related to University work as school education it is remarkable how unwilling University workers are to express themselves publicly on the subject, how strong a tendency they have to make it a matter of private conferences or even merely to accept the accomplished fact. It may be urged that an individual teacher is naturally unwilling to pose as the spokesman of the academic point of view; but it may be answered at once that unless academic workers are prepared to state what they *think* is the academic point of view, the academic point of view will never be stated. The notion that the interests of investigation can be served by official machinery, guarded by a "hush-hush" policy instead of being ventilated by open discussion and criticism, is one that will not stand examination for a moment.

Clearly, the public propagation of the academic attitude will involve not merely support of certain extra-academic activities but also opposition to others—if only because of the strength of the anti-cultural tendencies in existing commercial society. The necessity of this opposition is very little recognised by University workers. Ignorant journalists and business men may make a cockshy of the University, but University men do not retaliate or, for the most part, even defend themselves. The academic view of the Press and of contemporary commerce could well be expressed in such terms as would shake up these vested interests, some of their more corrupt proceedings could easily be exposed by research workers—as apparently happens from time to time in the United States—but the University workers of the British Empire, at least, seem to "know their place" too well to engage in any such vulgar controversies. It is unnecessary to enter here into the reasons for this state of affairs. But clearly what is needed first and foremost in the way of University reform is something that will stiffen the backs of academic workers, that will strengthen the academic forces within the University and make it function more actively in an academic way. It should not need further demonstration that, whatever may be the "privacy" required for research, the academic life as such is not a secluded one, in the sense of being apart from social struggle. If it were, then, in these times, it could only perish.

For the reinforcing of the academic character of the University it is, of course, essential that the entrance standard should be maintained. The insistence on an entrance qualification which will be a guarantee of genuine cultural attainments, involves opposition to "reform" along any of the lines currently suggested; *e.g.* the provision of options in line with "up-to-date" (*i.e.* business) requirements, the acceptance of schoolmasters' recommendations, or the substitution of "tests" for examinations in which candidates have to show an ability to *discuss*—for it is above all in consecutive discussion that the possession of a cultural background can appear. With this is connected the serious question of the multiplication of Faculties. Here it may be contended that to bring within the University studies which are generally pursued in technical colleges of one kind or another, is to ensure that a high scientific standard will be maintained in investigations in these departments; it may incidentally be argued that the first three years' work in such courses as those in Agriculture and Veterinary Science in Sydney University is substantially equivalent to that required for a B.Sc. degree. Nevertheless, there is much to be said for the view that the standard in all "technical" faculties would be improved if all students had to begin by taking a degree in Arts or Pure Science; and it may be added that it would operate strongly for the advancement of science if Science students had a more thorough cultural training. At any rate, it is worth noting that it is especially in connection with the more "practical" Faculties that attempts are made to lower the entrance standard. The strengthening of the academic forces in the University, then, involves the renewed recognition of the central position of Arts and Pure Science, as well as the desirability of a closer correlation between the two; and it involves, in Sydney, the restoration of the authority of the Faculty of Arts in regard to conditions of entrance.

A further important requirement for the keeping up of academic standards is an increased measure of control of Universities by those who work in them. Indeed, it may be said that, until Universities are run by University staffs (with the delegation of a share of responsibility for the conduct of their studies to the students themselves), they will never be notably academic. This, of course, would involve a tremendous alteration not merely in present forms of organisation but in present attitudes. It is remarkable that, though the governing bodies of most Universities are obviously ill-equipped for the management of educational activities, educational workers in their employment show the greatest reluctance to criticise their actions and policy, and especially to do it publicly. How this "employee" complex can be thrown off it is not easy to see; but, at any rate, academic progress, if it does come about, will go hand in hand with the assertion by educational workers of their greater knowledge of the requirements of an educational institution than is possessed by the members of the legal, medical and other professions who are now in control. (Here, again, disunion and confusion of outlook are fostered among the staff by the multiplication of Faculties.) One sign of a more vigorous attitude on the part of University teachers would be the adoption, by those of their number who sit on the governing body, of the policy of full discussion with their colleagues of the transactions of that body. Considerations of "propriety" have little compatibility with a serious struggle for specific objects; and such considerations would soon cease to have weight, if University teachers became thoroughly convinced of the need for academic autonomy. The main force which may stimulate such a conviction is the growing invasion of the Universities, and concurrent undermining of their authority, by business interests.

It is in connection with the question of University government that the proposals put forward by the officials of the recently formed Sydney University Graduates' Association are most strikingly reactionary. The suggestion that the present small representation of the staff on the governing body should actually be reduced is thoroughly in accordance with the desire to have a businesslike University (in which a clear line would be drawn between directors and employees), and it is not surprising that these reformers pursue their aims with the professed motive of "service", which is the regular justification for every form of social interference. It is true that graduates *could* serve the University; they could do so by securing public support for the claims of academic independence, for the control of academic institutions by academic persons, in short, for the *democratic* working of these institutions. Indeed, with greater academic freedom, an increasing number of graduates would take this line. But, as it is, the generality of graduates have little understanding of either democracy or the academic life, and the abovementioned proposal of a greater measure of external control is an illustration of this fact.

It may be urged that it is a reproach to Universities if their graduates do not adopt a cultural outlook, and that this shows that some reform is urgently required. Undoubtedly that is so, but, as has been suggested, it is connected with the weakness of Universities on the academic side and cannot be corrected by a further weakening in that direction. In particular, it is not to be expected that the products of the narrower professional schools would exhibit much interest in culture. But, even under the best of conditions, the mere possession of a degree would not be a guarantee of culture or, incidentally, of an understanding of how a University should be run. The question is not of the receipt of so much culture, which may then be retained as a possession, but of *ways of living*. Even in passing through the University a student does not necessarily become deeply immersed in the academic way of life ; for the most part he has his professional prospects in view, and he remains apart from the permanent work of investigation, with which training for the professions is more or less harmoniously conjoined, but which no one will deny is an essential part of the University's activity. On the other hand, when he leaves the University, he does become immersed in a professional or commercial way of life, and may easily lose any sympathy he ever had with the academic life. Thus the notion that a graduate is fitted by his degree to contribute fruitfully to the working of the University is an entirely false one. It is well known that graduates' associations, particularly in the United States, have actively assisted in the commercialising of University life, and the same attitude appears in the Sydney proposals. The complaint that employers in general look askance at graduates is quite beside the point. There is little doubt that the main reason for this suspicious attitude is just that employers in general are opposed to freedom and grudge the University such freedom as it retains. And the notion that the University should alter its way of working in order to meet such complaints and suspicions is simply grotesque. As has been said, graduates in their professions and organisations and social life generally, could do something to propagate culture and stimulate recognition of the claims of the academic life ; any graduates who do so may reasonably expect sympathetic cooperation from academic workers. But those who demand a hearing merely on the ground that they are graduates, are thereby showing their *lack* of culture.

Some remarks may be made here on the lecture system, on which a number of ill-considered criticisms have been passed. Obviously there can be good lectures and bad lectures—but it would be foolish to imagine that any system could be devised which would rule out the possibility of bad work. Tutorials and seminars could also be badly conducted, and there is nothing to show that they are bound to stimulate the student to more active thinking. Indeed, one might suggest that the demand for tutorials springs rather from the desire for an extended and more effective " spoon-feeding ". If students think over the lecture-material for themselves and discuss it with one another, they will make more progress than if they are continually running to the staff for further explanations. The printing of lectures would not meet the situation at all; the question is of the gradual unfolding of a position and the growing understanding of it on the part of students—an understanding which is, of course, assisted by the working of class exercises, but which does not in any case keep exact step with the lectures, as if one should say, "I have grasped that; what is the next step?" A further misunderstanding of the process of learning is shown in the contention that students cannot be intelligently taking in the material if they are writing all the time. Anyone with any experience of public lecturing knows how little is taken away by those who simply sit and display an "intelligent interest", and how greatly understanding is enhanced by the practice of note-taking; he knows also how little intelligence there is in most of the questions asked "on the spot". There is no denying that improvement in many degree courses could be wrought, for example, by a reduction in the number of subjects to be taken, by an increase in the staff to meet the requirements of "practical work", and by the provision of definite channels whereby students could express their criticisms of the courses they attend—such increased freedom for students being something that would naturally develop along with academic freedom in general. But lectures, whether in the form of presentation of unpublished material, criticism of published works, or simply the consecutive treatment of leading questions with which all students of the subject must come to terms, will always remain the central feature of any course of higher study.

There will, of course, be all the greater a tendency for students to think for themselves, if it is recognised that the University is not just a collection of professional training schools and, consequently, that the provision of lecture-courses is only one part of its work. The character of the University as an institution for learning will never receive due recognition, until those who are devoted to learning are prepared to uphold it as an independent social force, and to engage in a struggle *against* commercialism. Naturally, the representatives of academic autonomy will have to come to various arrangements with other social forces, but they will always get the worst of the bargain unless they make their independence a condition of the arrangements. This applies particularly to arrangements with the State. The University can demand State support on the ground that it does prepare for the professions and that it alone, precisely because of its disinterested approach to the questions involved, can do so efficiently. It follows, however, that it cannot accept State control of the administration of funds supplied by the State, or any State interference in its policy. The forces which go by the name of "the State" do not understand the conditions necessary for the maintenance of academic standards, and have a definitely commercialist bias. Current proposals for examination "reform", for the establishment of country colleges, and the like, illustrate these facts, and show the necessity for a consistent academic resistance to all attempts to *cheapen* education—which is the essence of the commercialist outlook. Only an aggressive policy can enable the University to maintain its standards in these times; and such a policy, with the greater academic freedom which it would entail, would stimulate the support of graduates instead of allowing them to drift easily into the commercialist camp.

The question of the University's attitude to the State is closely connected with the question of patriotism. It will naturally follow from the above considerations that the University cannot be officially patriotic or, at least, that, where it is so, it is departing from the academic point of view. In any department of Political Science, as well as in departments of cognate studies, patriotic and anti-patriotic views will be considered, and certain conclusions arrived at as to the working of patriotic sentiments in political affairs. But in an institution in which investigation is paramount, patriotism must be regarded as subject to investigation and criticism, and allegiance cannot be owed to institutions in which investigation is *not* paramount. The same applies to all questions of acute social controversy. In a developed University, Marxist and anti-Marxist views, religious and anti-religious views, the theories of Freud and theories opposed to Freud, will be thoroughly gone into, and the outcries of excited business men or anxious parents will be vigorously and not apologetically met. This means, as already indicated, that the upholders of investigation cannot keep their work entirely within the confines of a single institution but inevitably become involved in public controversies. In particular, they are forced to interest themselves in the whole educational system, and to support freedom of investigation in the schools as well as in the University. And, in general, their opposition to the subjection of the University to business interests carries over into every department of social life. It is an interesting fact, in this connection, that no considerable academic opposition to governmental censorship has hitherto been aroused; but what it indicates is the comparative weakness of the academic interest, the spirit of investigation, in the Universities themselves.

It should always be remembered that culture is opposed not to commerce but to commercialism; the question is whether or not investigation is to be subordinated to the making of profits. As previously suggested, there is not only no opposition between science and industry, but the two are of the greatest assistance to one another. It was in Ionia as a centre of commerce that science first developed, and it is in the more industrially advanced countries that it still holds the leading place. But the mutual assistance between the two forms of activity depends on investigation not being subordinated to "utilitarian" requirements. Indeed, the further development of industry at the present time depends on the passage from the standpoint of the consumer to that of the producer, from the disorganisation occasioned by profit-seeking to the re-organisation consequent on socialisation. The attempt to impose a utilitarian outlook on the University is thus in line with the degeneration of present-day industry as indicated by unemployment, crisis and war. And, on the other hand, the upholding of academic standards is in line with the regeneration of industry by the productive forces which socialisation will release.

It is not the object of this article to work out in detail the position here outlined. The point of immediate importance is that the academic forces are confronted by the forces of commercialism, and that the struggle against the latter can be conducted effectively only if the upholders of investigation take account of the general condition of society, the state of social forces and the direction of their movements. It is not enough that they should simply investigate; for the carrying out of investigation, the establishment of investigation as a way of life, depends on social conditions and involves relations of assistance and resistance to other forms of activity. And, of course, it is just because they are subjected to these opposing forces, because not merely the institutions they work in but they themselves carry the opposition within them, that investigators lose sight of the issues and have to be awakened, if investigation is to continue, to the dangers confronting them. But social orientation is precisely one of the conditions of culture; and thus we have another argument against narrow specialist training and in favour of the dissemination of culture, in the sense of a general grasp of the scientific, artistic and social activities of mankind, and the direction of one's own work in relation to these. The advancement of such culture, and that alone, is what can be seriously meant by University reform.

DISCUSSION.

THE LOGIC OF RELATIVITY.

By HERBERT C. CORBEN.

IN an article recently published in this Journal⁽¹⁾ Mr. C. C. Allen condemns the principles of the theory of Relativity as unsound, non-existent, unnecessary, untrue, and meaningless, claiming " in short, that relativity is a delusion ". Whilst not presuming to write a defence of Relativity, for the theory is defended by its own successes, I should like to point out one or two misconceptions in Mr. Allen's paper.

In the first place it is said that " in his book on Relativity, Einstein does not make reference to even one single observation. The whole of his reasoning is based on purely imaginary happenings." In Chapters XIII and XVI of this book,⁽²⁾ however, are mentioned four experiments, by means of which it has become possible to decide between the theories of Newton and Einstein, in favour of the latter. Two of these—experiments on aberration and on cathode rays—are only mentioned, but the other two are fully described. I refer to Fizeau's experiment and the justly famous Michelson-Morley experiment, without which Relativity would certainly be the delusion that Mr. Allen considers it now.

Secondly, Einstein's assertion of the equivalence of gravitational and centrifugal forces is criticised on the ground that " centrifugal force always acts outwards from the centre of rotation, while a gravitational field always exerts an attraction inwards towards the centre of mass ", a fact which Einstein himself is careful to point out.⁽³⁾ Einstein is not asserting the equivalence between the space distribution of the fields, but only between their effects at any particular point. He is identifying the forces themselves and not the way in which the magnitudes of these forces change as we move from point to point. He is saying that, if we were to experience a force of, say, ten pounds weight in a certain direction, we would not be able to find out, from the nature of the force itself, whether it were caused by gravity or by rotation.

On page 12 of his book, Einstein considers a raven flying in the air uniformly and in a straight line and viewed by two observers, one in a moving train, and the other on an embankment under which the train is passing. This illustration is criticised on the grounds (*a*) that the raven may be flying with zero velocity with respect to the train ; (*b*) that the magnitude and direction of the raven's velocity are different for different observers. The former is merely a limiting case, included in

Einstein's general remarks, as "motion uniform and in a straight line" is understood to cover also this special case of zero motion. The latter objection is both irrelevant and inconsistent, as we shall see, with Mr. Allen's own statement that Einstein's first principle is merely a statement of Mill's principle of the Uniformity of Nature.

As Einstein's four statements of the principles of Relativity have been criticised, I state them below for convenience of reference. The first two are the basis of the special theory, the second two of the general.(4)

(1) Natural phenomena run their course with respect to K′ according to exactly the same general laws as with respect to K, if relative to K, K′ is a uniformly moving coordinate system devoid of rotation.

(2) In addition to K, all bodies of reference K′ should be exactly equivalent to K for the formulation of natural laws, provided that they are in a state of uniform rectilinear and non-rotary motion with respect to K.

(3) All bodies of reference K, K′, etc., are equivalent for the description of natural phenomena, whatever may be their state of motion.

(4) All Gaussian coordinate systems are essentially equivalent for the formulation of the general laws of nature.

Mr. Allen remarks that (1) is "merely a restatement of Mill's principle of the Uniformity of Nature", for "when we say that natural phenomena run their course we say all that it is possible to say unless we observe and describe the phenomena in detail". Surely, however, to say that natural phenomena run their course without reference to any coordinate system is meaningless. Measurement can be made only with respect to some such system, and to speak of quantities that cannot be measured, even in theory, is useless.

Now as stated by Mill himself,(5) the principle of the Uniformity of Nature is that natural phenomena run their course in such a way that "whatever is true in any one case is true in all cases of a certain description; the only difficulty is to find what description". It might be objected that no frame of reference is mentioned in this statement, but this is so only because, for obvious reasons, the earth is implicitly taken as this frame. The phrase "with respect to the same coordinate system" is in most cases implied in the phrase "of a certain description". Obviously, if our raven were considered on two occasions when actuated by the same motives, it would appear different if in one case the observer were on the earth and in the other on the train. In fact, as we have seen, Mr. Allen brings forward practically this in his criticism of Einstein's treatment of the case.

However, Mr. Allen identifies (1) with Mill's principle on the ground that (1) may be read "Natural phenomena run their course", which is, of course, inconsistent with the above result, indicated by the example that Mr. Allen quotes, that there is no meaning in such a statement. Thus the objection (*b*) is inconsistent with the identification of (1) with Mill's principle.

Einstein's statement includes Mill's, but it goes much further. From it, together with the experimental fact of the constancy of the velocity of light, is built up a beautiful theory that would never appear from Mill's principle alone, important though that principle may be.

Regarding Mill's principle, as Mr. Allen rightly says, as "axiomatic", we see that Einstein's second statement is exactly equivalent to his first, and is not the "totally different formulation" it is said to be. For (1) may be read "Natural phenomena run their course with respect to all bodies of reference K′ according to exactly the same laws as with respect to K, if K and K′ are moving uniformly without rotation with respect to each other", which is equivalent to a combination of Mill's principle and (2).

Einstein's statement (2) is criticised on the ground that it is obviously untrue for one particular case (*viz.* when the reference frames K and K′ are moving away from each other with the velocity of light), and hence that it can be altogether condemned. However, in the first place, such a case is impossible, not only practically, but theoretically, for, according to Relativity, a body's mass tends to an infinite value as its velocity tends to that of light, so that two bodies can never have a relative velocity equal to that of light. Moreover, the formula that leads to this result for the variation of mass with velocity has been directly verified by the experiment with cathode rays mentioned above.

Despite this experimental evidence, it might be argued that it is illogical to use a result of the theory of Relativity in order to justify one of the fundamental principles of the theory itself. If, however, we deny this evidence, we still see that the case under consideration is not such an exception as might be thought.

Mr. Allen supposes that one of the reference frames (K′, say) is the earth, and the other, K, is moving away from the earth with a velocity equal to that of light. His objection to this case is, then, that "to an observer on this reference body nothing would appear to happen on the earth, a moving body would not appear to move, and a falling body would not appear to fall". However, in the same way that motion along some path in space would not appear, so also there would be no motion in time. From the point of view of the observer, there would be no such thing as progress of time on the earth. He would regard nothing as happening on the earth, not in a finite time, but in no time, which is quite consistent with our general principles.

We see, then, that this case leads to a result that, although indeterminate, is not inconsistent with the other cases. If, in (2), by "exactly equivalent" is meant "not inconsistent with", then the case we have considered leads to no condemnation of Einstein. If, however, it means something more than this, then Mr. Allen is right in his criticism of (2) in that the case we have considered should have been excluded in the statement of it.

No arguments are advanced against the first statement (3) of the general theory beyond the remark that it depends on the untrue special theory. We hope to show that the special theory follows of necessity from certain facts and plausible hypotheses, so that this objection is not valid.

There is a tendency to interpret Einstein's phrase "equivalent to" as "congruent with", which leads to a difficulty in following Einstein's third statement. Thus it is remarked that "supposing an observer to be seated in an aeroplane which is rushing towards the earth in a spinning nose dive and which is at the same time subjected to a gale of varying intensity, it can hardly be claimed that the records of the observer will be equivalent to those of an observer on the earth's surface". Obviously, of course, the records of the observer in the aeroplane are not congruent with those of the observer on the ground, but they are equivalent to them in that neither set is more fundamental than the other.

Now, if we assume Mill's principle, statement (3) could be read "Natural phenomena run their course with respect to all bodies of reference K, K′, etc., according to the same natural laws", which Mr. Allen would interpret, on analogy with his interpretation of (1), as a restatement of Mill's principle of the Uniformity of Nature. Hence, on Mr. Allen's own interpretation, Mill's principle, together with (3), gives Mill's principle alone. Thus (3) cannot be condemned as untrue, for it has led to no inconsistency. It must be either true or meaningless, although Mr. Allen criticises it as being unjustified. We see, of course, that the alternative "meaningless" arises from exactly the same fallacy that led to the identification of (1) with Mill's principle. This does not, however, leave us with the certainty that the other alternative "true" holds, but it leads us to the result that the statement that the theory is untrue is inconsistent with Mr. Allen's criticism of the theory.

Einstein's final statement (4) can be understood only in the light of mathematics; indeed, without mathematics, to understand Relativity at all is a practically impossible task. We may, however, consider the special theory logically, as a good example of the method of induction.

If we assume the existence of an ether, we know that light is propagated with respect to it with a definite velocity, usually denoted by the symbol c. Now this ether is either stagnant (*i.e.* rigid) or mobile. For reasons that will appear later, we wish to show that the earth is in any case moving with respect to the ether immediately surrounding it.

If the ether is stagnant, the earth must be moving through it, for otherwise the earth would be at the centre of the universe, an idea too flattering to receive serious consideration. If the ether is mobile, the earth, on moving through it with a velocity v, say, drags it with either a velocity v (so that there is no relative motion between the earth and the ether that is in contact with it) or with some velocity less than v (as postulated by Fresnel). The former case, however, is inconsistent with Arago's experiments on aberration and Fizeau's experiment, which show that, if classical mechanics (as opposed to relativity mechanics) holds, and if a body be moving through the ether with a velocity v, it drags the ether with some velocity less than v, depending on the nature of the body.(6) Thus the ether immediately surrounding the earth is moving with respect to the rest of the ether with this velocity, whilst the earth itself is moving with the greater velocity v with respect to the stagnant part of the ether. Hence in any case the earth is moving with respect to the ether immediately surrounding it.

Both Einstein's theory and Fresnel's ether-drag theory accounted for the experiments of Arago and Fizeau mentioned above. The *experimentum crucis* deciding between the two was the Michelson-Morley experiment, described on pages 52 and 53 of Einstein's "Relativity". We have seen that, on the ether-drag theory, there must be relative motion between the earth and the ether, but this experiment completely failed to detect this motion, despite the fact that the apparatus was sensitive enough to detect an effect many times smaller than that expected. The only explanations of this result so far advanced are (*a*) that the velocity of light depends upon that of its source; (*b*) the Fitzgerald-Lorentz contraction hypothesis; (*c*) the Special Theory of Relativity. (*a*) has been refuted by de Sitter's experiments on binary stars, and is in any case inconsistent with the wave theory of light. (*b*) contains a double weakness. Not only does it explain the Michelson-Morley experiment on the basis of an unjustified assumption, but it fails to account for the results of the cathode ray experiments without the addition of an even further arbitrary hypothesis.

Thus, in order to explain and describe natural phenomena as revealed to us so far by experimental research, we must either make these two quite unjustified assumptions, or adopt Einstein's special principle of relativity. This does not, of course, prove that Einstein's theory is the only possible one, but it shows that it is practically as certain as the result of Michelson's experiment, which work by Kennedy and Illingworth, with more sensitive apparatus, has verified very well.

Unfortunately the general theory does not admit of logical treatment as does the special theory—it can be developed, even in its earlier stages, only in the language of mathematics. None but the mathematician can read any meaning into Einstein's statement (4), none but the mathematical philosopher can pass judgment on the truth or falsity of it.

The general theory is a more or less natural extension of the special theory, but it is based on experimental evidence, some of which is none too certain. It explains the three Einstein effects, *viz.* (1) the rotation of the perihelion of Mercury ; (2) the deflection of light by a gravitational field ; (3) the deflection of spectral lines towards the red. Of these, the first and third are now considered as practically certain, although there is still some doubt about the second. The world awaits with interest further experimental researches that will make the evidence conclusive, either one way or the other.

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(1) *A.J.P.P.*, Vol. XI, No. 4 (Dec., 1933), p. 293.
(2) Einstein: "Relativity. A Popular Exposition", Methuen, 1922.
(3) *Ibid.*, p. 80, note.
(4) *Ibid.*, pp. 13, 61, 97.
(5) J. S. Mill: "A System of Logic", p. 354.
(6) Actually this velocity is equal to $v(1-a^2)$, where a is a constant depending on the nature of the body (the reciprocal refractive index).

RESEARCHES AND REPORTS.

I.

THE RELATION OF ASCENDANCE-SUBMISSION TENDENCIES TO NEUROSIS.[1]

By FREDA FARRAM.

THIS investigation was carried out on the suggestion that the Freudian theory of neurosis might be inadequate to explain many personality maladjustments, which could more simply be accounted for by the Adlerian concept of desire for assertion and dominance. Two tests of the questionnaire type were used, one designed to indicate neurotic trends and the other intended to demonstrate assertive and submissive tendencies. The tests were the Personal Inventory, after Thurstone,(1) and the Ascendance-Submission Test of G. W. and

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[1] Research carried out in the Psychology Laboratory, University of Sydney.

F. H. Allport,⁽²⁾ and were administered to a group of Second Year Psychology students, comprising 60 men and 56 women, at the University of Sydney. The results of the tests were correlated to find the degree of correspondence between the scores, and each test was given twice, with an interval between presentations of eight weeks, to determine its reliability.

The Tests.—The Personal Inventory Test consists of questions taken from Thurstone's Personality Schedule. The original inventory contained 223 questions, designed to discover neurotic tendencies in freshmen at the University of Chicago, and after investigation the 42 questions most indicative of neurosis were selected as an abbreviated scale. For the present work the test was adapted for local conditions by changing the phrasing of some of the questions; in addition one question was omitted and another added. The subject is required to answer the questions by underlining the word "Yes" or "No" printed after each question. The answer "Yes" indicates neurotic tendencies except in four cases. Sample questions are:

6. Are your feelings easily hurt?
20. Do you have difficulty in making friends?

The A-S Reaction Study of G. W. and F. H. Allport is a much longer test. There are two forms—one involving 33 situations for men and the other with 35 situations for women. There is a greater range of replies, most of the situations having three alternative responses from which to choose. The questions in the A-S Reaction Study outline particular instances instead of the vague general questions of the Personal Inventory Test. Sample situations for both sexes are:

2 (*a*). When you see someone in a public place or crowd whom you think you have met or known, do you inquire of him whether you have met before?

Sometimes ———
Rarely ———
Never ———

2(*b*). Are you embarrassed if you have greeted a stranger whom you have mistaken for an acquaintance?

Very much ———
Somewhat ———
Not at all ———

The two forms of the test have been independently standardised and score values have been carefully computed for each choice with groups of 400 men and 200 women. A positive score indicates responses which show ascendance, while a negative score denotes submissive reactions.

The reliability from the correlation of the first and second presentations was in both tests clearly marked, although the groups were relatively small.

	Reliability Coefficients.	
	Personal Inventory.	Ascendance–Submission.
Men	+ ·86 ± ·02	+ ·84 ± ·03
Women	+ ·91 ± ·02	+ ·85 ± ·03

These correlations are higher than those gained by other investigators. Thurstone reports a split half correlation for the Personality Schedule of ·897. The reliability of the A-S Test as found by Broom is ·718, measured by a retest; while for this test Allport gives ·74 for men and ·78 for women.

Results.—The correlations between the two tests were not so high because, although they both aim at studying the adjustment of the individual to his social environment, they do so from different aspects. The correlation coefficients for the two presentations were as follows:

	Correlation Coefficients.	
	$PI_1A\text{–}S_1$.	$PI_2A\text{–}S_2$.
Men	+ ·598 ± ·06	+ ·578 ± ·06
Women	+ ·518 ± ·07	+ ·436 ± ·08

These figures were then corrected for "attenuation", using the formula

$$r = \frac{r_{A_1B_1}}{\sqrt{r_{A_1A_2} \quad r_{B_1B_2}}}$$

and the resulting correlations were ·7024 for men and ·6693 for women.

These results differ widely from those of other workers. Ross Stagner,⁽³⁾ in 1932, in "an attempt to clarify some of the inter-relationships of tests which allegedly measure similar or related factors in personality", used among others the A-S Test and the unabridged Personality Schedule. With a group of 75 students of the sophomore, junior and senior classes at Gustavus Adolphus College at the University of Wisconsin, Stagner obtained a correlation between these tests of – ·514 ± ·057.

In 1934 Herbert Moore and Isabel Steele(4) gave the A-S Test and the complete Thurstone Personality Schedule to 58 students at Mount Holyoke College. The report of their work in the *Journal of Abnormal and Social Psychology* is contradictory. In one place the correlation is given as ·521, in another as – ·521; but as they infer "a marked correlation between submissiveness, introversion and maladjustment" it is probable that their correlation was positive. Allport obtained a low positive correlation, ·347, with the Thurstone Test.

It is difficult to explain these varying results, except on the ground that the scoring of the A-S Test was, in the first two cases, slightly modified by converting scores into decile ranks to permit of the scores for the two sexes being used in the same correlation. For the present work the two groups were treated separately. Also for the three investigations mentioned the complete Thurstone Test of 223 questions was used instead of the abridged form.

The scores show some interesting features.

	PI$_1$.	PI$_2$.	A–S$_1$.	A–S$_2$.
Men—				
Range	1 to 26	0 to 30	+50 to –46	+54 to –38
Average Score	10·97	9·05	+2·18	+8·19
S.D.	6·37	4·81	17·21	24·67
Women—				
Range	1 to 25	1 to 33	+47 to –27	+52 to –24
Average Score	13·29	12·09	+7·34	+11·02
S.D.	5·67	6·69	15·32	19·35

Although in every case the second presentation shows a higher average score, *i.e.*, one tending further from neurosis or submission, it is interesting to note that the range in the Personal Inventory Test extends further towards the lower end in the second test, indicating that on reflection the subjects at the lower or neurotic end tended to rate themselves even lower. This tendency, however, is not common to both tests.

Conclusions.—From the results of the correlations obtained we may conclude that the traits measured by the

Personal Inventory Test and the Ascendance-Submission Test have some decided common element.

The Personal Inventory Test was designed to indicate neurotic tendencies and, of the numerous questions originally suggested, the most indicative have a definite correlation with Allport's A-S Reaction Study. The A-S Test studies the ascendance and submission of individuals in relation to their fellows, involving the Adlerian idea of the desire for power as an important element in our lives.

Although Thurstone found that the answers to the sex questions included in his original schedule differentiated clearly between neurotic and normal subjects, none of the 10 sex questions were included in the final list of most differentiating questions. Apparently they were not as indicative of potential neurosis as were the 42 other questions, which were finally selected.

It seems evident, therefore, that a more general explanation of neurotic trends is indicated in the Adlerian concept of desire for power and dominance over others than solely in the more narrow Freudian view of sex.

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(3) Ross Stagner: Intercorrelation of Some Standardised Personality Tests. *Journal of Applied Psychology*. Vol. XVI, No. 5.

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II.

AN ASSOCIATION SCALE?

A Preliminary Investigation.[1]

By HENRY H. FERGUSON.

(*a*) *Introduction.*—The Kent-Rosanoff free association test, reprinted in "A Manual of Psychiatry", edited by A. J. Rosanoff, formed the starting point for the preliminary investigation described here. The test referred to consists of 100 common words administered according to the word-list method and frequency tables (based on the responses of 1,000 normal American adults) for use in evaluating responses obtained.

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[1] Carried out in the Psychology Laboratory, University of Otago, N.Z.

(*b*) *Subjects and Method.* — Forty-four University students attending the First Year Class in Psychology at Otago University.

These subjects were submitted to the test in small groups and wrote their responses. The time taken for the response was not recorded.

(*c*) *Results.*—Given below is a table of total frequencies of each subject's 100 responses, as determined on the basis of the frequency tables of Kent and Rosanoff. For example, a subject who responded with the word "chair" when "table" was given was credited with 267, that being the frequency of that response as given in the Kent-Rosanoff test. Where the response was not contained in the frequency table, S. was credited 0.

TABLE OF TOTAL FREQUENCIES OF EACH SUBJECT'S 100 RESPONSES.

Score.	*Frequency.*
18,999–17,000	2
16,999–15,000	1
14,999–13,000	12
12,999–11,000	14
10,999– 9,000	7
8,999– 7,000	4
6,999– 5,000	3
4,999– 3,000	0
2,999– 1,000	1

The association scores show a tendency to distribute themselves in a normal curve. The actual curve obtained is skewed toward the upper limit, the mode lying between 12,000 and 13,000, and the mean being 11,842·4. The correlation—r—between what may be termed association scores and class marks is ·297 ± ·094.

(*d*) *Consideration of Results.*—The results show (i) that even with "normal" subjects the range in association scores as derived from such a table is very great, (ii) that association scores appear to distribute themselves according to a normal curve, (iii) that the correlation between association scores and class marks is non-significant and that probably the correlation between association scores and I.Q.'s is also quite low. Such findings appear to the author to justify the belief that association scales analogous to the scales obtaining in intelligence tests could be devised and that we could say, *e.g.*, of John Smith, not only that he has an I.Q. of 115, but also that he has an A.Q. (association quotient) of 100.

One practical value of such a scale might be that it could give us (when considered in relation to the actual I.Q. determined) an indication of the extent to which an I.Q. is unreliable. Each I.Q. is followed by a plus and a question mark; we cannot tell to what extent, if any, certain inhibitive emotional factors are at work.[1] Perhaps an association quotient could give us some indication.

(*e*) *Reflections on the Word-List Method Considered.*—While the author believes that it would be well worth while attempting to devise an association scale based on the word-list method, he would like to point out that words have various disadvantages: (i) they are not international, (ii) their meanings change, (iii) their associations are influenced by (*a*) geographical setting and (*b*) changes of custom. And he would like to suggest, from observations with locally constructed material, that varied "ink-blot" designs, after the fashion of the Rorsach test, might form a more nearly universal basis for a scale such as is suggested in this article. In constructing such material the author used what he believes to be a new method. Kymograph paper was blackened, varnish was dropped on it, in such a way as to fix the carbon on areas varying in size and shape, and the unfixed carbon was removed. That done, each sheet was run through varnish, leaving a glossy surface with certain designs. Such designs (or clusters of designs) were then mounted on suitable cards.

REVIEWS.

PSYCHOLOGY AND EDUCATION. By C. R. McRae, M.A., Ph.D. Third Edition, 1934. Sydney: Whitcombe & Tombs, Ltd. Pp. 426.

The third edition of Dr. McRae's *Psychology and Education* includes a chapter on the psychology of Alfred Adler. His doctrines are deftly summarised, with special reference to their educational implications. Dr. McRae is no worshipper of Adler, but regards his "eloquent advocacy of a training which is social and co-operative" and "his insistence upon the all-important pedagogical principle that a teacher's prime duty is to build up the child's faith in himself" as an important contribution to educational theory, while "for psychology, his main services have been to provide a useful corrective to the Freudian emphasis upon the sex-instinct, and to show the close relation between mental health and social orientation".

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[1] Another important related problem arises in the case of subjects with high intelligence quotients and high scholastic performances. We would like a higher degree of certainty in forecasting their success, say, as lawyers or medical practitioners.

One may doubt, however, whether Adler's psychology is really of much help to the educationalist. We can quite well recognise the importance of co-operation without accepting the theory of the "ego-ideal", or the "masculine protest", or of any special "inferiority complex" or "superiority complex" as distinct from the more general theory of satisfaction and dissatisfaction, with the further recognition that hallucinatory satisfaction ("superiority complex") tends to be sought if genuine satisfaction is impossible. The really fundamental distinction for a theory of co-operation is that made by Freud between repression and sublimation, these relationships holding between forces located in different minds (*e.g.,* in the minds of teacher and pupil) just as much as between forces in the same mind, this inter-relation between psychological and social theory being one of the most important points brought out by Socrates in *The Republic.* Incidentally, it is hardly fair to Freud to describe his theory even as "up to a point, anti-social". He is certainly opposed to many existing social tendencies, but we cannot call his theory anti-social unless, assuming that repression must occur in any human society, we infer that the attempt to remove repressions is an attempt to destroy society—an inference which Freud would not accept.

If we do reduce Adler's doctrine to a support of co-operation, it seems doubtful whether he has made any contribution at all, apart from giving an authoritative backing (which will not be authoritative if we reject the main tenets of his doctrine) to that "old-fashioned ethics, good but platitudinous" which Dr. McRae finds in his work. Adler does take up educational questions more specifically than Freud, but this is not to say that Freud's work is the less valuable to the educationalist nor is it true, as is sometimes maintained, that its importance is restricted to the treatment of "problem children". The theory of transference, for example, is of fundamental importance in any discussion of the process of education.

The additions to the chapters on "Transfer of Training" and "The Measurement of Emotion and Character" do not affect the main argument of those chapters, but increase the already extraordinary comprehensiveness of the book by further references to experimental work. The chapter on "Attention and Interest" contains some additions of a pedagogical character, emphasising the use of surprise in teaching, the humorous nature of Dr. McRae's examples at this point being themselves an excellent illustration of his thesis.

J.A.P.

CONCERNING YOU AND ME. By C. R. McRae, M.A., Ph.D. Humphrey Milford, Oxford University Press, Melbourne, 1934. Price, 4s. 6d.

If Dr. McRae's *Psychology and Education* has been called "Psychology Without Tears", this book should be called "Psychology is Great Fun". The book is written with a light and graceful touch, and is lit by a really gay spirit. It is joyfully conceived and replete with amusing allusions. The chapters run as follows: "We Eat", "We Fight", "We Grow Afraid", "We Seek to Know", "We Acquire Possessions", "We Make and Build", "We Sleep", "We Fall in Love", "We Bring Up Children", "We Get Together", "We Assert Ourselves But Also Submit", "We Play", "We Laugh"; and the promise of these captions is fully maintained. Nearly every chapter begins with an appropriate limerick. If, from this, you judge the book to be merely giddy, delirious and trifling, you will be quite wrong; and if, for that reason, you pass it by, you will miss a treat and much useful information as well. All the useful information which parents and teachers need to know about the child's instinctive equipment is here, most pleasantly expounded and suitably illustrated. Anybody who reads the book will certainly increase his knowledge of humanity, for beneath the skilfully humorous presentation is much important information and wise comment. The reviewer intends to recommend it strongly to his students, and the book would be admirable, too, for parent education, for which the time is overdue.

H. T. L.

LA SPIRITUALITA DELL'ESSERE E LEIBNIZ. By G. E. Barié. Pp. 553. Cedam. Casa editrice Dott. Antonio Milani già litotipo. Padua, 1933. Price, L. 57.

Signor Barié's real subject, as he explains in the preface to his work, is "La spiritualità dell' essere con particolare riguardo alla filosofia leibnizia". The first section of the book (pp. 1-85) consists of a discussion of Leibniz's predecessors, the second section (pp. 85-327) is wholly devoted to Leibniz, while the last section (pp. 327-519) consists of an exposition of "la logica dell' attuarsi".

Leibniz's predecessors are discussed only in so far as they contributed to the theory of the spirituality of existence. Barié's treatment of Bruno and of the Renaissance generally is, however, of special interest to the English reader. The

Renaissance combined, he argues, an intense emphasis on the importance of human power and a denial of Mediæval anthropomorphism. From this affirmation and from this denial modern idealism derives, denying as it does that the world was made for man and yet basing itself upon the activity of the human spirit. Bruno is important because he provided a metaphysical backing for the theory of Copernicus, and at the same type rejected the Aristotelian distinction between matter and power. Matter is not something inert upon which force is exerted from without, but is itself a centre of power. His theory is thus analogous to that of Leibniz, differing from it in that the centres of power are not regarded as being *intelligible.*

Descartes' theory is full of vacillations, since he sees in thought the only source of truth, but does not realise that it is the only truth. All philosophers after Descartes are endeavouring to resolve, each in his own way, the dualism that resulted from Descartes' uncertainty.

Leibniz himself is treated very fully: the whole course of his philosophical development is discussed, and his intricate relations with his contemporaries are fully detailed. Especially, Barié recognises fully the importance of physics to the philosopher—a fact which is especially clear in the philosophy of Leibniz, where the position of motion and energy is so prominent, but which has to be taken account of in the development of any philosophical position. Barié argues that Leibniz's philosophy progressed in certain directions more than in others, and that, especially, remnants of his earlier Cartesian theories explain his adherence to the position that substances cannot communicate among themselves. This view, while it was a necessary consequence of the theory of independent substances, was no longer a necessity when he had developed his theory of monads, and its retention in his theory prevented him from developing a thoroughgoing philosophical position. Leibniz, then, never understood that "la spiritualità dell' essere non significa solo trovare nel mondo interiore la ragione del mio io, ma anche la ragione delle cose di cui il mio io è necessario termino correlativo" (p. 545).

Barié follows in the footsteps of Croce and Gentile in so far as he can be described as an insurgent Hegelian. He wants to retain the Hegelian theory of the creativeness of the human spirit, while abandoning the dialectic. Thus he hopes to retain the principle of contradiction—that mysterious law which is at once an identity, needing no proof, and a fruitful principle from which theories can be deduced. A principle so useful to metaphysicians must surely be retained.

Croce and Gentile, however, also come in for some criticism. Barié desires to incorporate more of the material of Leibniz and Kant than have the post-Hegelians. Much that was valuable, he thinks, was too hastily thrown away along with the rejection of Kant's dualism. His own reconciliation of being and becoming appears in the statement that "L'essere è il punto di vista per il quale il pensiero vede la necessità logica di abbracciare il tutto nella sua unità; il divenire è invece quello per cui lo stesso pensiero vede l'essere nei suoi gradi o momenti" (p. 470). This is not, he argues, a dualism because the two points of view find their expression in the same individual in accordance with its own thought as subject or reflection on its own thought as "autoconscienza", both being as much thought as the other. This reconciliation of unity and individuality appears in the series C_1, C_2 . . . C_n by which C becomes E, the terms of this series being individual reals and yet being also possible, as activity or becoming—and forming part of a total reality CE, described elsewhere as the "segment", C causes E. Summing up, he argues that "as real, each one constructs his own subjective world as an affirmation of himself; as possible, each forms part of one great universal force, which comprehends in itself everything possible as its moments without any dissolution of continuity; and thus the whole is logically, an actual absolute, but each as real yet compulsively thinks of it from his point of view as opposed to himself, and for that reason determines the spatial and temporal world" (p. 470).

Now, if Barié had meant only to distinguish between a series and the members of that series, and to point out that we can consider that series as one thing, while being at the same time able to distinguish parts within it, then he would be working towards a correct theory of being and becoming, but this does nothing to show that there is one total series, of which everything forms a part. Further, it is not a question of looking at things through different eyes, or of seeing things immediately and mediately, but simply of seeing things as they are, as being (*i.e.*, as persistent) and as becoming (*i.e.*, as having stages), or as both *continuous* and *"momentary"*. This admission that a thing is both whole and part will not imply that there is one all-embracing whole; in fact, it will actually contradict this view and will involve a realist recognition of things as on the one level. Continuity and divisibility are bound up together, so that the recognition that a thing is continuous implies that it has various moments (not, of course, that it is dissoluble into those moments) and equally the recognition that a thing is a moment implies that there is something to be divided, *i.e.*, that there is continuity. Against the Idealist conception of two classes—*the* continuous (absolute) and *the* divided (historical things), the second being moments of the first, while the first is not a moment of anything, the realist supports the doctrine of things as both continuous and divisible. We have not, then, to explain away either persistence or change, but simply to recognise that both are always present, this being bound up with the recognition of things as spatio-temporal.

This problem of being and becoming is, of course, related to the other Idealist difficulty, the problem of evil, since becoming is regarded as *imperfect* as opposed to the perfection of being. Here Barié seems to think that the only alternative to idealist doctrine is exploded Utilitarianism. But, again, if we adopt the ethical position that things are good quite irrespective of who wants them, parallel to the general realist position that things are what they are whoever knows them, we are certainly not bound to accept Utilitarianism, and yet, equally, we shall not have to *reconcile* free will and determinism, this question being quite irrelevant on this type of theory. The "problem of evil" will simply not arise any more than the "problem of becoming", in general. We shall simply take the view that some things are good and some are evil—that this is a historical fact—and we shall not say that one is on any different level from the other. The problem of evil only arises if we accept the doctrine of a special creative moral agent (human or divine), and will have no place in a theory of things as both active and passive.

Barié's formulation of idealism is interesting, as displaying, once again, the trend away from Hegel. Unlike Carlini, he seems to know nothing of the work of the English post-Hegelians, but he seems, like the members of that group, to be troubled by the amount of explaining away that is necessary in Hegelian theory. At any rate, he has produced a very complete and thorough account of Leibniz from the idealist's standpoint, and although the realist naturally considers that other sides of Leibniz's work are more worthy of emphasis, Signor Barié's encyclopædic knowledge reveals much that is of general interest.

J. A. PASSMORE.

THE HEROIC WORKER. By F. R. Worts. London: Harrap. Pp. 180. Price: 2/-.

Containing thirty short biographies of English "heroic workers", this volume is intended to be a history reader for twelve-year-olds. Empire builders, scientists, industrial organisers, thinkers and artists are included.

The work is written in a simple, straightforward manner, though in places the naïveté of its jingoistic sentiments is somewhat disconcerting (*e.g.* pp. 18–19, 67–68, 170).

Encouraging the study of personalities, the work has little claim to be regarded as historical, for it affords no direct study of social forces. It is a slightly "democratic" advance on the old-style history-texts which presented "history" in a series of royal biographies.

J. W. CAMPBELL.

NOTES AND NEWS.

An informal meeting of persons interested in the advancement of logical studies, and particularly in the possibility of providing greater facilities for the publication of papers in symbolic or mathematical logic, was held on 27th December of last year at New York University during the annual meeting of the Eastern Division of the American Philosophical Association.

Professor C. J. Ducasse reported that a considerable majority of the logicians and mathematicians who answered a *questionnaire* recently sent out, were of the opinion that a need existed for an international journal of logic which would supplement existing channels for the publication of papers in symbolic or mathematical logic. The sense of the meeting was that an association should be organised for the advancement of logical studies through the establishment of such a journal, and possibly also by means of meetings occasionally held in conjunction with those of the A.A.A.S. or the American Philosophical Association; and that a committee of organisation be appointed by the chairman. Some 48 persons present at the meeting indicated their willingness to join such an association.

A committee of organisation, consisting of the following persons, was appointed: C. A. Baylis, Philosophy, Brown; A. Church, Mathematics, Princeton; M. R. Cohen, Philosophy, C.C.N.Y.; H. B. Curry, Mathematics, Penn. State; C. J. Ducasse, Philosophy, Brown; S. K. Langer, Philosophy, Radcliffe; J. B. Rosser, Mathematics, Princeton; H. B. Smith, Philosophy, Univ. of Pa.; P. Weiss, Philosophy, Bryn Mawr.

Since more definite plans for the proposed journal must depend largely on the funds that would be available, all persons (whether or not themselves working in the field of Logic) who would favour the launching of such a journal, and who would be willing to lend it their support by joining the proposed association (with dues of probably $3 a year) are asked to send their names to Professor C. J. Ducasse, Brown University, Providence, R.I.